EXPERIMENTAL

AND

CLINICAL RESEARCHES

ON THE

PHYSIOLOGY AND PATHOLOGY

OF THE

SPINAL CORD,

AND SOME OTHER PARTS OF THE

NERVOUS CENTRES.

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If physicians knew how much the practice of medicine and surgery, as well as the science of physiology, may progress from researches made on living animals, that is from *vivisections*, I have no doubt that many would enter in this field of enquiry, and that they would be soon repaid for the trouble of experimenting, by making important discoveries.

It is from experiments on living animals that Dr. Jobert, de Lamballe, Prof. S. Gross, and some other surgeons have deduced the principles of treatment of wounds of the bowels; it is in experimenting on animals that Amussat has discovered the torsion of arteries, which every day proves so useful in man; it is by researches made on living animals that Prof. D. Brainard has discovered many of the important surgical facts that are related in his admirable Prize Essay. I might point out many other striking proofs of the usefulness of vivisections, but I think that my readers may themselves find these proofs if they

will examine what has been done lately about diabetes, albuminuria, eclampsia, epilepsy, the diseases of the pancreas, the effects of poisons and the treatment of poisoning, etc. They will find in this examination what a wonderful progress there has been in all these parts of medical science, coming from experiments made on living animals.

Not only is it easy to make discoveries in performing such experiments, but the number of discoveries to be made after a single experiment may be extremely considerable, as will be seen hereafter.

Before entering into the details of this paper, I think proper to say that when I speak of a complete transversal section of a lateral half of the spinal cord, I, of course, mean a transversal division of all the elements of this half, *i. e.*, the gray matter and the three white columns: the anterior, the lateral, and the posterior.

Part I.

Researches on the place where the sensitive and motor nerve-fibres make their decussation in the nervous centres.

In the actual state of science the explanation of the fact that an alteration, in one side of the brain, produces a loss or a diminution of sensibility and of voluntary movements, on the opposite side of the body, is that the motor and sensitive nerve-fibres go up to the encephalon, along the spinal cord, without having any decussation, and that, when they have arrived at the medulla oblongata, they begin to decussate, and they continue do so all along the middle line of what is now called the *Isthmus of the Encephalon* (medulla oblongata, Pons Varolii, Crura Cerebri, Corpora Quadrigemina, etc.).

I will try to prove: 1st. That this view is entirely erroneous; 2d. That the sensitive nerve-fibres make their crossing almost exclusively in the spinal cord; 3d. That the voluntary motor fibres decussate mostly in the inferior part of the medulla oblongata.

§ I. *Effects of the section of a lateral half of the spinal marrow on sensibility.*

Until lately, some experiments made by Galen had been generally considered as proving that there is no *crossing* of action in the spinal cord. My experiments on animals, and some pathological facts I have collected, are partly opposed to the view attributed to Galen. I have found that if it is true that almost all the motor nerve-fibres do not cross each other in the spinal cord, it is quite the reverse for the sensitive nerve-fibres, so that if an alteration of the *right* side of the spinal

cord exists, it produces a loss or a diminution of voluntary movements on the right side of the body, and a loss or a diminution of sensibility on the *left* side, and *vice versa*.

There is in reality no great difference between the results of my experiments and those of Galen's experiments, as far as we know what they were. Galen, as the reader may ascertain,* speaks of the paralysis of voluntary movements, but he does not say a single word about sensibility. And it is, nevertheless, upon the falsely understood results of Galen's experiments that, for a great many centuries and even until the last few years, physicians have based the universally admitted opinion that there is no crossing of action in the spinal cord, neither for sensibility nor for voluntary movements!

The facts which I have found and which prove that there is a crossing of action for sensibility in this nervous centre, are the following:

1st. If a lateral half of the spinal cord is divided transversely at the level of the tenth costal vertebra, on a Mammal, it is found that sensibility is much diminished and, in some cases, entirely abolished in the posterior limb opposite to the side of the section. On the contrary, the sensibility, far from being lost, appears to be much increased, in the posterior limb, on the side where the section has been made.

2d. If, instead of one transversal hemisection of the cord, two, three or some more are made, on the same side, the same effects are observed.

3d. If, instead of mere sections, a removal of a lateral half of the spinal marrow is effected, the same results are still obtained. For the performance of this experiment a longitudinal section, one inch in length, is first made in the median plane of the cord, and then two transversal sections on one side are made at the extremities of the longitudinal one, so that a part of the cord is completely separated from the rest and removed.

4th. If the lateral section is not complete, and if the part left undivided is in the neighborhood of the centre of the cord, it is found that sensibility appears to be increased in the posterior limb on the same side, and that in the other posterior limb there is only a slight diminution of sensibility. If the part left undivided is considerable, sensibility does not appear to be diminished in this last limb, and sometimes it seems rather increased.

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* See his following works: De locis affectis, Lib. iii, cap. xiv, and De anatomicis administrationibus, Lib. viii., sect. 6.

5th. If, in performing the section of a lateral half of the spinal cord, the instrument goes a little too far and divides also a small portion of the other half in the central part, then the posterior limb on the side of the complete section is less sensitive than in the normal state, and the posterior limb of the opposite side loses completely its sensibility.

6th. If the section of a lateral half of the spinal cord is made at the level of the second or third cervical vertebra, it is found that sensibility becomes very quickly much greater in the parts of the body on the side of the section, and that, on the contrary, the parts on the other side become evidently less sensitive.

Before proceeding farther in the exposition of my experiments, I think necessary to examine here the results already related. It is clear that if the transmission of sensitive impressions were operated in the spinal cord, according to the generally admitted theory, we should find, after the section of a lateral half, sensibility lost or at least diminished on the corresponding side of the body and almost normal on the other side. It is exactly the reverse that we find: the side which should have lost its sensibility does not lose it at all,* and the one which should have kept its sensibility loses it almost entirely.

I conclude that the generally accepted theory is wrong, and that there appears to be a crossing of action for sensibility in the spinal cord. The following experiments are still more decisive proofs that this is the real state of things.

7th. If after a section of a lateral half of the spinal cord at the level of the eleventh costal vertebra, we perform the section of the other lateral half, at the level of the sixth costal vertebra, so that the two lateral halves of the cord are cut transversely, we find that sensibility is entirely lost, or very nearly so, in the two posterior limbs. Sometimes a very slight degree of sensibility remains, more particularly in the posterior limb on the side where the spinal cord has been divided at the level of the sixth costal vertebra.

8th. If two sections of lateral halves are made as in the preceding experiment, but at a greater distance, one from the other, for instance, one on the right side, at the level of the eleventh costal verte-

* Far from being lost, sensibility appears, as I have already said, to be much increased. This hyperœsthesia is a very curious fact, of which I will speak in another part of this paper.

bra, and the other on the left side, in the cervical region, nearly the same results are obtained as regards the posterior limbs, but the sensibility is increased in the right anterior limb and it remains, though much diminished, in the left anterior limb.

9th. If, after having divided transversely a lateral half of the spinal cord, in the neck, at the level of the roots of the second pair of nerves, we lay bare the very sensitive nerves going to the ear, in dogs or rabbits, we find that their sensibility, on the side of the section of the cord, appears increased, and that, on the contrary, on the other side, they appear either destitute of sensibility or very slightly sensitive.

10th. Sections of a lateral half of the medulla oblongata give, as regards sensibility, the same results as sections of a lateral half of the spinal cord.

11th. If a longitudinal section be made on the part of the spinal cord giving nerves to the posterior extremities, so as to divide that part into two lateral halves, then it is found that sensibility is completely lost in the two posterior limbs, although voluntary movements take place in them. This is one of the experiments of Galen, but I repeat that he does not speak of sensibility.

12th. If a similar separation of the two lateral halves of the spinal cord be made on the whole part supplying nerves to the anterior limbs, then we find that sensibility is lost in both these limbs, and that it is only slightly diminished in the posterior limbs.

13th. If the same operation be made as in the preceding experiment, and if afterwards a transversal division be made on one of the lateral halves of the cord in the part where the longitudinal section has been performed, then we find that the posterior limb on the side of the transversal section remains sensitive, and that the other posterior limb loses its sensibility.

I do not think necessary to stop to show that these experiments are all in accordance, to prove that the transmission of sensitive impressions made on one side of the body takes place, at least for a great part, along the opposite side of the spinal cord, and that there is, consequently, a crossing of the sensitive nerve-fibres in the spinal cord.

My experiments have been made on animals belonging to many different species, and particularly on dogs, cats, sheep, Guinea pigs, rabbits, marmots, etc. Many of these experiments have been witnessed by Messrs. Rayer, Flourens, Magendie, Lallemand, Duméril, Cl. Bernard,

Ch. Robin, and the Academy of Sciences of Paris has given me a reward of three hundred dollars for some of these researches.

To ascertain the degree of sensibility in my experiments, I have employed various modes of excitation: mechanical, galvanic, physical, (*i. e.*, warmth and cold) and chemical.

A proper use of galvanism and of a red hot iron are the best means of ascertaining the presence and the degree of sensibility. When a slight galvanic current is employed, it gives no pain if applied to limbs in which sensibility is much diminished. In making use alternately of galvanic currents of different energies, we are enabled to find what is the degree of sensibility of the different parts of the body.

I have never judged of the degree of sensibility of a limb by its movements when I irritated its skin, because these movements might have been merely *reflex*. My means of judging have been the shrieks of the animals and the movements of their ears, their faces and their necks.

I have constantly compared the degree of sensibility of the parts of the body situated behind the injured part of the spinal cord, with the sensibility of the anterior parts of the body and particularly of the face. By so doing, it is possible to ascertain the changes of sensibility resulting from the operation on the spinal cord.

The following example will show how we may appreciate the degree of sensibility. The lateral half of the spinal cord having been divided transversely on a dog, at the level of the 10th costal vertebra, I apply a galvanic excitation of moderate intensity on the face, and the animal shrieks a little and moves its head and tries to get away.

The dog is left quiet for some time, and then I apply the same galvanic excitation on the posterior limb, on the side of the section, and the animal shrieks a little more than in the preceding case and it tries more decidedly to get away. Now, after it has become quiet again, the same current is applied to the posterior limb, on the opposite side, and there are no shrieks and no attempts to go away. The dog appears not to feel this excitation.

Sometimes I ascertained the degree of sensibility by a totally different means: I gave chloroform to animals having had a lateral half of the spinal cord divided in the cervical region, and I found that the slight sensibility existing in the limbs, on the side opposite to the section, disappeared almost at once, and that, on the contrary, a relatively long time passed before the production of a complete anæsthesia in the

limbs on the side of the section. Generally anæsthesia appeared in the face before appearing in these last limbs.

There cannot be, I think, any doubt that my experiments prove that there is a crossing of sensitive nerve-fibres in the spinal cord, but there are some questions that they do not so clearly solve, and such are the following. Do all the sensitive fibres cross each other in the spinal cord? and if some do not, what is their proportion to those which do pass from one side of the cord to the other? I believe that the truth is that very nearly all the sensitive nerve-fibres, coming from the trunk and limbs, cross each other in the spinal cord, and that, in consequence, the transmission of the sensitive impressions, made upon one side of the body, takes place almost entirely along the opposite side of the spinal cord. If this conclusion is not sufficiently grounded on the facts, heretofore related, I hope that the pathological cases I am about to mention will contribute much to confirm its exactitude.

§ II. *Pathological facts proving that there is a crossing of action for sensibility in the spinal cord.*

After having discovered, by experiments on animals, that there is positively a crossing of action in the spinal cord, I have, at first, been very much surprised that physicians had not discovered the same fact on man. But, after having thought for some time about it, I have found: 1st. That the number of cases in men of an alteration of the spinal cord, limited to a small part of a lateral half, is not considerable; 2d. That many physicians being unaware that an alteration of one side of the spinal cord is able to produce a loss of movement on the same side and a loss of sensibility on the other side of the body, have not examined the state of sensibility of their patients in the side where it was altered. This will be proved by some of the cases which I will report hereafter.

I will divide into two series these pathological cases. In the first series I place cases of limited alterations of a lateral half of the cord, some proving the existence of the crossing of sensitive nerve-fibres in this organ, some others in accordance with the preceding but not so able to prove this fact. In the second series, I place cases of alteration of one side of the medulla oblongata or of the Pons Varolii, which cases seem to me to prove positively the existence of the crossing of sensitive nerve-fibres in the spinal cord.

First Series of Cases.

In the name of a committee of the Société de Biologie of Paris, I have published a report on a paper of Dr. Oré of Bordeaux, in which he has recorded two important cases, observed by himself, and of which the following is an abstract.

Case I.—A patient was admitted into the *St. André* hospital in Bordeaux, under the charge of Professor Gintrac. The symptoms were: paralysis of voluntary movement on the right side of the body, with conservation of sensibility; on the contrary, conservation of voluntary movements on the left side, and a great diminution of sensibility. The morbid appearances consisted in the presence of a fungoid growth (*végetation fongoïde*), pressing upon the right lateral half of the spinal cord.

Case II.—Another patient was admitted in the same hospital. *Symptoms:* Loss of voluntary movements, with conservation of sensibility in the two limbs on the left side. On the other side sensibility much diminished (*trés obtuse*). *Inspection*: A clot of blood was found in the lateral half of the spinal cord in the cervical region.

Case III.—A man, after having felt a sudden pain in his back, became paralyzed of voluntary movements in the right inferior limb. Sensibility was not altered on this side, but on the left side, where movements were not impaired, sensibility was entirely lost from the breast to the foot. *Inspection:* Brain and its membranes normal; in the spinal cord a hæmorrhage had taken place, and blood was found in the right side of the central gray matter, in the neighborhood of the anterior column, in the costal region. This case is assuredly a very remarkable one, and as it has been recorded by a very accurate observer, Mr. Monod, a surgeon of eminence in Paris, we cannot have any doubt about the exactitude of the details.*

Case IV.—Mrs. W., after a profuse hæmorrhage, became paralytic. Upon one side of the body there was a loss of sensibility, without, however, any corresponding diminution of power in the muscles of volition. The breast, too, upon that side partook of the insensibility,

* See for a full account of the case the Bulletin de la Société Anatomique. No. xviii., p. 349, obs. 3.

although the secretion of milk was as copious as in the other. She could see the child sucking and swallowing, but she had no consciousness from feeling that the child was so occupied.

Upon the opposite side of the body there was defective power of motion, without, however, any diminution of sensibility. The arm was incapable of supporting the child; the hand was powerless in its grasp; and the leg was moved with difficulty, and with the ordinary rotatory movement of a paralytic patient; but the power of sensation was so far from being impaired that she constantly complained of an uncomfortable sense of heat, a painful tingling and more than the usual degree of uneasiness from pressure, or other modes of slight mechanical violence.

She again proved pregnant. Her delivery was easy; but after about ten days she complained of numbness on both sides. Her articulation was indistinct; she became more and more insensible, and sank completely comatose.

No positive disorganization of the brain could be detected. The ventricles, however, contained more than usual serum; and there were found thickening and increased vascularity of the membranes, with firm adhesion in some parts; in others, an apparently gelatinous, transparent and colorless deposit interposed between them.

Unfortunately, no examination of the spinal cord was made.

This case has been recorded by Dr. H. Ley, who communicated it to Sir Charles Bell*. It cannot prove much, but, if besides what we know of the symptoms, as regards sensibility and movement, we take notice of what is said about the temperature of one side of the body, it acquires more importance. I will show, in another part of this paper, that an alteration of a lateral half of the spinal cord is followed by an increase of temperature, in the limb or limbs situated behind and on the side of the alteration. It seems that the same thing existed in Dr. Ley's patient, who was constantly complaining of an *uncomfortable sense of heat*, in the side paralyzed of movement.

Case V.—A man fell on his back, from a height of 20 feet; after having recovered his consciousness, he discovered that the whole left side of his body, from the shoulder to the foot, was paralyzed of movement,

* See: The nervous system of the human body, by Sir C. Bell. 3d Ed. Lond. 1844, p. 245.

but that there was not the slightest alteration of sensibility, and that the right side of the body, in which the movements were free, was completely deprived of sensibility.

Three important facts, precisely like those I have discovered in animals, after the transversal section of a lateral half of the spinal cord, existed in this case.

1st. A morbid exaltation of sensibility in the side where movement was lost.

2d. A diminution of temperature in the side where the paralysis of sensibility existed.

3d. An increase in temperature in the side where the paralysis of movement existed.

When Dr. Dundas, who has recorded this curious case, published it, the patient was living and improving; so that we do not know what was the alteration existing in the spinal cord, but the analogy between this case and other cases, where inspection took place, and with the results of my vivisections, appear to prove that there was an alteration of a lateral half of the spinal cord.

In the following case the direction of a sword, which had cut a part of the spinal cord, in a man, appears to prove that a lateral half had been divided, and the symptoms have been the same as in the animals, upon which I have experimented.

Case VI.—A drummer of the National Guard of Paris, received a wound in the back of his neck. A sword, thrown at him, had penetrated the superior part of the right lateral half of the neck. An incomplete paralysis of movement took place in the right side of the body, and, some time after, it was *accidentally discovered* that sensibility was lost in many parts of the left side of the body. After twenty days the wound was cured and the man went out of the hospital, still paralysed.

From what we know of this case, it appears that the sword had incompletely divided the right lateral half of the spinal cord. The paralysis of motion on the right side of the body was certainly produced by the division of a part of the anterior column, and as the instrument had penetrated the right side of the back of the neck, it must have divided the parts between the anterior column of the spinal marrow and the external surface of the right side of the neck. These parts, besides the muscles and bones, are the lateral and posterior columns and

the gray matter of the right half of the spinal cord. So that, in this case, nearly the same injury and also the same morbid phenomena had existed as in the animals on which I have divided a lateral half of the spinal cord.

In this case the surgeon, and it was Boyer,* had not examined the state of sensibility in the side where the patient had the power of movement, and as the patient himself was not aware that he had lost his sensibility (and such a fact is not of uncommon occurrence) he had not spoken of it. Only the 13th day after he had been admitted in the hospital, having been pinched by a nurse who was playing with him, he found that he had lost his sensibility and then the surgeon, (who, as every one knows, is one of the best practitioners who have existed in France, in this century,) was informed of the fact.

What has occurred to Boyer, in this case, shows that I was right in saying, that physicians, not being aware that there is a crossing of sensitive nerve-fibres in the spinal cord, have neglected to examine the state of sensibility, in certain cases, where, had they done so, they would have found it lost or diminished, and would have been, in consequence, led to discover the crossing of action for sensibility. In the following case, for instance, there is not a single word said, about the state of sensibility in one side of the body, where, according to what I have discovered, sensibility was very likely diminished. Dr. R. B. Todd, who relates this case†, is, undoubtedly, one of the most eminent and the most accurate Physicians of London, and I regret to have to point out this fault, in a case about which he has displayed, in other respects, his usual talent of observation.

Case VII.—M. C. admitted Nov. 18th. Headache. Complete paralysis of the left arm, with flaccidity, and some wasting of the muscles. In walking she dragged her left leg with a sweeping movement. Although the palsy of the arm was complete as regards motion, she retained some power of the leg and could move it slowly up and down, as she lay in bed. But she was unable to hold it up from the bed. Sensibility was impaired, although she retained a considerable amount of feeling on the paralyzed side. On the 11th of December she was found to have lost the use of the left leg completely. She complained

* See his Traité des Maladies Chirurg. Vol. vii. p. 9. 1st ed.

† See his Clinical lectures on Paralysis, disease of the Brain, and other affections of the nervous system. London 1854, p. 321. Case LXIII.

also of twitching on the left side of her body. The intercostal and abdominal muscles of the left side remained perfectly motionless during respiration. A remarkable sign indicated the want of equilibrium between the right and left abdominal muscles in expiration; the umbilicus, at each expiration, was distinctly drawn to the right side. Sensibility was not more impaired in the paralyzed limbs than on her admission. No difference could be observed between the two sides as regards temperature. On the 12th the right arm was found to be partially paralysed; she was just able to flex and extend the finger slowly and feebly.

The action of the diaphragm was so feeble that it could scarcely be felt in its descent. On the 13th, sensation more impaired in the left arm and leg, and she complained of a want of power over the right leg. On the 14th, the paralysis continues to increase; it is complete on the whole of the left side, below the neck and of the right arm. Voluntary power over the right leg rapidly failing; she can but just move the toes and contract the muscles of the calf, but not sufficiently to move the foot. Breathing gradually diminished and she died.

The disease consisted chiefly in an enlargement of the odontoid process of the second vertebra. This extended backwards, wearing through the dura mater, and it was covered at its upper part and on the left side by a fibro-cartilaginous growth, which compressed and flattened the spinal cord on the left of the median fissure. The compression of the cord was so great, that it seemed as if a large portion of the nervous matter had been pushed from the left to the right side and partly upwards; and the cord was swollen both above and to the right of the compressed part. The nervous matter on the left side was soft and slightly discoloured, as from small ecchymoses; that, on the right of the fissure, was soft and diffluent.

I have thought proper to record here all the principal features of this case because it *appears* to be opposed to my view about the crossing of sensitive fibres, in the spinal cord. But 1st, as I have previously said, the state of sensibility of the right side has not been noted in this case; 2dly, the alteration of the spinal cord, even many days before death, was not limited to the left side.

I regret not having been able to procure the *Ephemeridæ Naturæ Curios.*, which contain, according to Abercrombie, a case in which

there was loss of motion on the one side, and loss of feeling without any diminution of motion on the other. (Cent. II, Obs. 196.)

I read in Bostock's Physiology, (3d edi. p. 163, note 2,) that a patient, whose case is reported by Dr. Bright, (Med. Reports, vol. II, p. 548-9,) had a paralysis of the motive nerves of the left extremity and of the sensitive nerves of the right. In this case there was, if I remember well, an alteration of the left side of the spinal cord. I had taken some notes about this case, but I have lost them, and I have not actually at my disposal, Dr. Bright's book.

Second Series of Cases.

The great fact that, when a disease exists in one side of the brain, it is the opposite side of the body, which is paralyzed, both of movement and sensibility, has compelled Physiologists to put forward some views as regards the place where exists the crossing of fibres. After the discovery of Sir Charles Bell, it has been impossible to explain by the decussation of fibres, existing between the two anterior pyramids of the Medulla Oblongata, the crossing of action for sensibility. Two opinions have been proposed, one by Sir Charles Bell, the other by Longet. According to the first of these writers, the sensitive nerve-fibres make their crossing, on the posterior surface of the Medulla oblongata, in a great part of the extent of the 4th ventricle.* According to Longet the posterior columns of the spinal cord, which he considers as containing all the sensitive nerve-fibres, coming from the body, pass through the cerebellum, from which they go out, as a part of the *processus cerebelli ad testes*, and they decussate in front of the Pons.†

In his very remarkable work on the nervous system, Foville‡ has described, very minutely, decussations of fibres, taking place all along the median line in the medulla oblongata, the Pons Varolii, between the crura cerebri, and between the corpora quadrigemina.

Certainly if the crossing of the sensitive nerve-fibres do not take place in the spinal cord, as I have tried to prove that it does, it must take place somewhere in the medulla oblongata, the Pons Varolii, etc. And if it takes place in the medulla oblongata and the Pons, what

* Philosop. Trans. of the Royal Soc. of London, 1835. And also the Nerv. syst. of the human body, p. 231-40. London 1844, 3d ed.

† Traité d'Anat. and de Physiol. du syst. nerv. 1842, vol. I, p. 385.

‡ Traité complet de l'anat. du syst. nerv. cérébro-spinal. Paris 1844, p. 298-326.

must we find in cases where an alteration exists only in one side of these nervous centres? There must evidently be a diminution of sensibility on both sides of the body, because many fibres, belonging to the two sides, must necessarily be altered or divided. Let us suppose, for instance, a tumor, as large as a walnut, having altered or destroyed one of the sides of the Pons Varolii (there are many such cases on record); and let us admit that it is the left side. Now, in this side, there are the sensitive fibres of the left side of the body, which have not yet made their decussation and which make it in the Pons or a little forwards, between the corpora quadrigemina or the crura cerebri. These fibres are altered or divided, and, in consequence, the left side of the body must lose a part of its sensibility. But, the fibres coming from the right side of the body, and which have made their crossing in the medulla oblongata, and also the fibres of the right side, which pass in the Pons, from the right to the left, must be altered or divided, and, in consequence, the right side of the body must lose a part of its sensibility: so that the two sides of the body, in this hypothesis, must have a diminution of sensibility. A like reasoning might be applied to what must take place when the disease exists in one side of the medulla oblongata, admitting that the decussation of sensitive fibres takes place mostly there, and, the same things might be said, also, for the parts anterior to the Pons Varolii, if it were admitted that there takes place the greatest part of the decussation of the sensitive nerve-fibres.

Let us see now what are the facts. In almost all the cases where a disease has existed, on one side of the medulla oblongata, the Pons, etc. there has been a loss or a diminution of sensibility in the opposite side of the body, and no diminution in the same side. In cases where there has been an alteration on both sides, (in one of the above named nervous centres) but greater in one than in the other, sensibility was lost in the side of the body opposite to the side of the nervous centre which was most altered, and only diminished on the side of the body opposite to the side less altered in the nervous centre.

All these facts clearly prove: 1st. That the sensitive nerve-fibres of the trunk and limbs do not decussate in the medulla oblongata, the Pons Varolii, etc.

2d. That they have made their crossing before reaching these nervous centres, and that, consequently, they make it in the spinal cord itself.

These conclusions result so positively from the facts I have spoken

of, and they are so decisive that I cannot understand that Physiologists and Physicians have not already deduced them from these facts.

I do not intend to relate here all the cases that are on record, and which prove that an alteration of one side of the medulla oblongata, or of the Pons Varolii, produces hemiplegia both of sensibility and movement. I will merely relate some which will serve as specimens. I ought to say that for the diseases of these nervous centres as for the diseases of the spinal cord, the recorders of the cases have very frequently neglected to speak of the state of sensibility, and have merely said that there was hemiplegia. So that these cases, which might have been of great scientific and practical importance, are deprived of their principal subject of interest.

All the most eminent physicians who have particularly studied the diseases of the nervous system agree in saying that the alterations of the medulla oblongata and of the Pons Varolii produce a loss of sensibility in the opposite side of the body and not on the corresponding side.

This being the general opinion I will relate merely a few cases.

Case I.—Headache on right side. Numbness and difficulty to move the left arm. Slight paralysis of the left leg. After some time, hemiplegia perfect in left side, where sensibility and temperature are diminished.

Inflammation and suppuration of the right side of Pons Varolii.*

Case II.—A man was admitted at the hospital of Bicetre. Mouth drawn towards the right side, tongue deviated to the left, pupil of the left eye dilated and almost deprived of mobility; no palsy in the limbs. Two days after, headache, right side; limbs paralyzed on the left side; sensibility very dull in these limbs. The next days the symptoms go increasing, but the limbs on the right side remain perfectly healthy. The hemiplegia becomes complete in the left side. Coma,—death.

Inspection: Brain and cerebellum healthy. In the right lateral half of the Pons Varolii, a white softening (*ramollissement*) is found; the left half is healthy. The centre of the softened part is extremely soft (*diffluent*) and water takes it away.

This very important case has been recorded by Mr. Charcellay and published by Ollivier d'Angers.†

* Hist. Anat. des Inflammations, par A. N. Gendrin, 1826. Vol. ii. p. 155.

† Traité des maladies de la Moelle épinière, 1837—3d ed. vol. ii., p. 315.

Case III.—G. J. was brought to a hospital deprived of consciousness, with hemiplegia of the left side, and loss of sensibility of the same parts. Pupils dilated, the left without mobility ; face drawn on the right. After bleeding he recovers his consciousness and is better for about a week. Then he loses the power of speech. Headache and the same paralysis of movement and sensibility on all the left side of the body, which is slightly flexed, but without rigidity. Coma,—death.

Inspection : Nothing in the brain. In the inferior and the middle part of the Pons Varolii exists a *ramollissement*, very distinct, irregular in its shape, large like an almond, rose-coloured. The softened tissue resembled to liquid pus.

This case is reported by Dr. L. Greuzard.*

It is known that, very frequently, organic diseases slowly developed, in the different parts of the encephalon, produce but a very slight degree of paralysis of movement and of sensibility. In such cases this slight paralysis exists on the side of the body opposite to the side of the alteration. I relate the following case as a specimen.

Case IV.—A gentleman had been for several months in bad health : Dyspepsia, liver complaint, tightness across the chest. Respiration very imperfect along the right side of the thorax, and some œdema of the legs. One morning he was suddenly seized with giddiness, noise and confusion in his head, and numbness of the whole right side. He was oppressed but not comatose. He complained of a tight and cramped feeling of his right arm and leg, with much prickling and loss of command of the parts, but when desired to grasp another person's hand with his, the muscular power did not seem to be diminished. Face slightly distorted.

He became better. His sight was good, but he had a slight degree of paralysis of the upper eye-lid of the right side. His breathing was easy, and he made no complaint except of the tight and cramped feeling, with numbness of the right arm and leg. Fever came and he died. He continued sensible to the last, and during this febrile attack he seemed to have acquired an increased command over the limbs of the affected side.

Inspection : Brain and cerebellum healthy. In separating the cere-

* Archives gén. de méd. 2d series. Vol. V., p. 458.

bellum from the Pons Varolii a cavity was exposed about the size of a hazel nut, lined by a soft cyst, and full of dark, grumous blood of a firm consistence. This remarkable cavity was formed partly in the substance of the Pons and partly betwixt it and the base of the cerebellum. It was decidedly more to the left side than the right, and the surrounding substance was softened.*

These cases agree with the results of my experiments, and I believe that if we take a survey of all the facts (experimental and pathological), that I have related, it is impossible not to conclude that almost all the sensitive nerve-fibres, coming from the trunk and limbs, cross each other in the spinal cord. Some of the fibres, belonging to the cervical part of the spinal cord, perhaps, decussate a little higher, *i. e.*, in the medulla oblongata; but a few only may be in this case, as it appears from one of the experiments above related (see exp. 9th).

I have, many times, examined if the roots of a pair of nerves make their decussation near or far from the point of their communication with the spinal cord. After a transversal section of a lateral half of this organ has been made (never mind at what level), if we irritate the posterior roots of the pair of nerves situated directly behind the section, we find that pain is felt by the animal, and if we irritate the roots of the same pair of nerves on the opposite side, the animal appears to feel much less. So that the decussation of the sensitive nerve-fibres takes place, at least partly, directly after the entrance of these fibres into the spinal cord.

The same result is obtained when, besides the section of a lateral half of the spinal cord, the posterior column of the other half is divided at the same place, and this proves decidedly that it is not through this part of the spinal marrow (I mean the posterior columns), that the transmission of sensitive impressions takes place. According to my researches it is through the central part of the cord that this transmission is made.

The pathological facts I have related, concerning the Pons Varolii, agree perfectly well with the following experiment.

A longitudinal section is made on a young animal, on the median line of the medulla oblongata and the Pons Varolii, so as to divide them entirely into lateral halves. It is certain that if the sensitive

* Pathol. and Practical Researches on Diseases of the Brain and Spinal Cord. By J. Abercrombie. Case CXIX.

nerve-fibres of the body made their entire decussation or a great part of it, in these nervous centres, we should find sensibility very much diminished, if not lost, everywhere in the trunk and limbs. But this is not the case: the animal remains, for some time, very sensitive.

Before concluding this part of this paper, I must say a few words about the results obtained by some physiologists, who have made some experiments like one of mine.

Sir A. Cooper, at the request of Dr. J. Yelloly, has made one experiment, on a dog, to ascertain what are the effects of the section of a lateral half of the spinal cord. Yelloly had been led to have this experiment performed, after having witnessed the case of a man, who had numbness and paralysis of the right side of the body, and in whom a tumor, the size of a hazelnut, was found lying on the left side of the Pons Varolii, sunk into it, and extended into the left pyramid of the medulla oblongata.* The experiment made by Sir Astley, which might have shown to him the curious and important fact that a section of one half of the spinal cord, produces a paralysis of movement on one side and a paralysis of sensibility on the other, did not lead him to such a discovery. He noticed only the state of voluntary movement, very likely, because he took for granted that what existed for movement ought to exist also for sensibility. And thus it is that, even sometimes, men of the greatest talent, taking things for granted, retard, instead of assisting, the progress of science.

Schoeps, Van Deen and Stilling, in examining, on animals, the state of the sensibility of the parts of the body situated behind a section of a half of the spinal cord, have found that it was not lost; but as they did not examine comparatively the state of sensibility of the other side, they did not discover the important fact that the sensitive nerve-fibres decussate in the spinal cord.

I had, myself, ascertained long ago the truth of the fact, found by Schoeps, first, and afterwards by Van Deen and Stilling; but I had, like these physiologists, given of it an explanation† quite different from that which I now admit. Kürschner and Longet have denied the fact that sensibility persists in the parts of the body situated behind and on the side of a lateral hemisection of the spinal cord. I have shown elsewhere what has led them to this erroneous statement.

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* Trans. of the Med. Chir. Soc. of London. Vol. I., p. 181.

† In my Inaugural Dissertation, 2d part, p. 20-28. Paris. January 3d, 1846.

Such was the state of science, as regards the results of a section of a lateral half of the spinal cord, when, in the year 1848, I made the discovery of the crossing of action for sensibility in the spinal cord. Since then, some of my experiments have been repeated, and found exact by Professor Oré of Bordeaux, and Dr. Ludwig Türck of Vienna.

§ III. *Influence of a section or an alteration of a lateral half of the spinal cord, and of the medulla oblongata, upon voluntary movements.*

I do not intend to examine at length this subject. I will only, after having briefly exposed the results of some of my experiments, show that there are many points of great physiological and pathological interest connected with the following question: in what part of the nervous centre do the voluntary motor nerve-fibres perform their decussation?

I have seen, so many times, that, in mammals, in birds and in other animals, the transversal section of a lateral half of the spinal cord is not followed by a complete loss of voluntary movements, in the parts situated on the same side and behind the section, that I have not the least doubt that there is, at least, in animals, a small number of motor nerve-fibres decussating in the spinal cord. But, in man, the facts *appear* to prove that none of the voluntary motor nerve-fibres decussate in the spinal cord. If it is so, there is every appearance that this decussation takes place *entirely* in the medulla oblongata.

This view is opposed to what is now generally admitted. Since the publication of the researches of Foville,* of Valentin,† and others, a general assent has been given to their idea that the nerve-fibres, coming from the antero-lateral columns of the spinal cord, after having ascended to the medulla oblongata, to the Pons Varolii, and higher, decussate there, on the median line.‡ And it has been concluded from this view, that the principal decussation of the motor nerve-fibres takes place in the Pons Varolii and between the crura cerebri, etc., and that the decussation of the pyramids is of less importance.

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* Loco cit.

† Traité de Névrologie, traduit par Jourdan, p. 236, 237, 246.

‡ Some anatomists have already dissented, and particularly L. Hirschfeld and Sappey, who admit that the transversal fibres of the Pons have been mistaken for decussating fibres. See the excellent *Traité d'anat. descript.* par P. C. Sappey. Vol. II., 1st part, p. 141.

But, I think, it is easy to prove that this view is not exact. Were it right, we should, undoubtedly, observe, in cases of alterations of one side of the Pons Varolii and neighboring parts, symptoms quite different from those which truly exist.

Let us examine what are the symptoms and what they should be if the theory, I consider erroneous, were right.

1st. Suppose an alteration on one of the crura cerebri; according to the theory, as a part of the decussation of the motor nerve-fibres takes place there, we should find that voluntary movements are diminished on both sides of the body, more, I acknowledge, on the side opposite to the alteration, but partly also on the same side of the body. This is not what exists. One side only of the body is paralyzed, and it is the opposite side. A number of cases prove that this is the rule. The hemiplegia may be complete or incomplete, according to many circumstances, and particularly the extent and the nature of the alteration, and the rapidity of its formation; but there is something constant, together with these numerous varieties, it is that the seat of the paralysis is on the side of the body opposite to that of the disease. It is evident, in consequence, that the decussation of the motor nerve-fibres has entirely taken place before they reach the crura cerebri.

2dly. The same thing may safely be said of the corpora quadrigemina. Although the cases relative to these organs are much less numerous than the cases relative to the crura cerebri, there are enough of them on record, to prove that the crossing of the motor nerve-fibres must have taken place entirely before they reach the basis of the corpora quadrigemina. Besides some other cases, there are two very interesting which have been published: one by Mohr,* and the other by Burnet.†

3dly. As to the Pons Varolii, the question is much more interesting, because this is the place where the decussation of motor fibres, according to Foville, Valentin and Longet, more particularly takes place.

Here, according to the theory of these distinguished anatomists, we ought to find different symptoms in these three different cases: 1st. Alteration limited to the superior part of the organ; 2d. Alteration limited to the inferior part (the nearest to the medulla oblongata); 3d. Alteration occupying the whole of a lateral half of the organ. In the

* In Casper's Wochenschrift, 1840, p. 497.

† Journal Hebd. de Méd., 1829, p. 439.

first case we should see an incomplete paralysis on both sides, but greater on the side of the body opposite to the side of the disease, and, in the second case, we should see also an incomplete paralysis on both sides, but greater on the side of the body corresponding to the altered side of the Pons, than on the opposite side of the body. Cases are on record proving that it is not so, and that whatever is the part of the Pons altered, (the superior, the inferior or the middle) the same effect is produced on voluntary movements. When paralysis is produced by the disease, it exists, exclusively, on the opposite side of the body, and when the alteration is not limited on one side of the Pons and extends on the other, then the side most paralyzed in the body is the one opposite to the most altered side of the Pons.

If the theory of Valentin, Longet and others were true, we should find in cases where the whole of one half of the Pons is diseased, the two sides of the body partly paralyzed,—the side opposite to the alteration less than the other. On the contrary, we find that paralysis exists only on one side, and that it is the one which, according to the theory, should be *less paralyzed*. I might prove that I am right by relating here many pathological facts, but as I have already mentioned some, I think it sufficient only to record the following.

Case I. G. was admitted in a hospital. The right side of the body bent; intelligence entire; all the left side of the face paralyzed both of movement and sensibility. Nothing anormal in the left side of the body; but on the right side the shoulder is falling, the superior limb is paralyzed. The inferior limb has still a part of its voluntary movements and of its sensibility.

The patient, after having been placed on his feet, could not stand, the right side giving way, and he should have fallen had not the trunk been supported. Respiration weak, difficult. Until death, the same symptoms have been observed, without modification.

Inspection: Spinal cord and brain denser than normally; this last organ is injected. Lateral ventricles contain five or six ounces of serosity. The whole left side of Pons Varolii is diseased; it is considerably larger than usual, forming a tumour which extends itself over the spinal cord and the crura cerebri and cerebelli, with the substance of which it is united. The tumour is composed of a fibrous tissue, possessing some of the characters of cancer. The posterior part of the tumour extends until the anterior parietes of the 4th ventricle. A

few small clots of blood existed inside of the enlarged and diseased left side of the Pons; another one, large like a bean, existed between the occipital bone and the right side of the Pons.

This important case has been published by Mr. Carré.*

The following case, related by Romberg,† is very much like the preceding, but more interesting, on account of a more precise account of the state of sensibility. I regret to be obliged to give only a short summary of it.

Case II.—A young Pole, after a nervous fever, became paralyzed of sensibility and movement in the left side. Anæsthesia in the right side of the face. Coma,—death.

Inspection: The right side of the Pons is the double of its normal size; it extends in front and behind, and there it passes under the right olive, compressing the neighboring parts of the encephalon and the roots of the nerves. The cause of the enlargement of the right side of the Pons was a very large clot of blood.

According to Romberg, a case very much like this one is recorded by Dr. R. Bright (Reports of Med. Cases. Vol. II., part 1, p. 49).

I will give now a case showing that, when an alteration exists in both sides of the Pons Varolii, but more in one than in the other, the paralysis is greater in the side of the body opposite to the most altered side of the Pons.

Case III.—Headache; both pupils dilated, still contractile, strabismus and diplopia. Diminution of movement and sensibility in the limbs, and particularly in the left ones. After a few days paralysis of the limbs increased.

Inspection: Nothing in the brain. Pons Varolii and right crus cerebelli, compressed and softened by a tumor, situated in the middle of the Pons, but more on the right side.‡

When the disease exists in the medulla oblongata, the symptoms vary according to its situation. Is it above the decussation of the pyramids, and limited to one side, it produces paralysis on the opposite

* Arch. génér. de Méd. 1st series. Vol. V., p. 234.

† Lehrbuch der Nerven krankeiten des Menschen. 1851. Vol. I., 2d part, p. 198, 202.

‡ See: Quoedam de Ponte Varolii. By H. Romberg. 1838, p. 17.

side of the body. Such a case has been published by Ollivier, d'Angers.*

When an alteration exists on one side, at the place where the pyramids decussate, we find that there is a partial paralysis on both sides of the body. This comes from the fact that there are some motor nerve-fibres belonging to the two sides of the body which are altered. Some curious cases of this kind are on record: one is related by Mr. Th. Chevalier.† There was a paralysis of the two arms, produced by a cancerous tumour on the summit of the left pyramid.

If the theory admitting the crossing of motor fibres in the Pons Varolii and in front of it were true, we should see, in cases of alterations limited to one side of the Pons, symptoms like those observed in the case recorded by Mr. Chevalier.

Now, to sum up what I have said about the parts constituting the *Isthmus of the Encephalon*, we see: 1st. That alterations, limited to one half of these parts, *i. e.*, the corpora quadrigemina, the crura cerebri, the Pons Varolii and the medulla oblongata (if the disease is above the decussation of the pyramids), produce paralysis on the opposite side of the body; 2d. That when the disease is at the place of the decussation of the pyramids, and on one side only, there is a paralysis on both sides of the body. It results clearly from these facts that the motor nerve-fibres coming from the trunks and limbs do not make any notable part of their crossing above the decussation of the pyramids.

Now the question is, whether all the motor nerve-fibres make their crossing where the decussation of the pyramids takes place. I have already said that it *appears* from the facts observed in man, that the motor nerve-fibres do not make their crossing in the spinal cord. If it is so (and I ought to say that I have some doubts about it) we arrive at a very strange result, of which I will say a few words.

Every one knows that on each side of the spinal cord there are three white columns: one anterior, one lateral, one posterior. The two first have been considered by some physiologists as being the only motor tracts of the cord, and it is true that their section produces a mere paralysis of movement, and that there are many pathological cases, in which these columns having been altered, there has been a paralysis of movement. But, experiments and pathological cases seem to prove that

* Traité des Mal. de la Moelle, 1st ed., obs. xxxvi.

† Lond. Med. Gaz. June 1834.

a paralysis of movement is more easily produced by a section or an alteration of the anterior columns than by a section or an alteration of the lateral columns. Now, many cases are also on record, proving that an alteration of the posterior columns may produce a paralysis of movement, so that, were we to conclude from these facts, we should admit that all the white columns of the cord are motor tracts. But I ought to say, that experiments on animals and some cases, in man, in which there has been only a transversal section or an alteration of the cord, not extended in length and merely destroying transversely the posterior columns, did not produce a paralysis of movement; so that we cannot admit that these columns contain voluntary motor fibres longitudinally disposed.*

The anterior and lateral columns appear then to be the only voluntary motor tracts in the spinal cord; but if they are so, and if there is no decussation in this organ, a very great difficulty presents itself: the crossing of the pyramids takes place only for the fibres of the lateral columns (not even for all of them), and the fibres of the anterior columns do not decussate in the medulla oblongata, so that there are certainly not more than one third of the voluntary motor nerve-fibres of the trunk and limbs, (and perhaps only one fourth) which decussate in the medulla oblongata, to form the anterior pyramids.† As all the

* In a paper on this subject I will try to prove that when the posterior columns are altered, in a great part of their length, there is a paralysis of the reflex movements and no decided loss of the purely voluntary movements. This reflex-paralysis may exist as well without as with a loss or a diminution of sensibility. There are many pathological cases on record, (I have collected more than twenty,) in which the posterior columns were more or less altered, in a great part of their length, and, in these cases, sensibility was entire, or only slightly diminished, and a reflex-paralysis existed. These facts certainly prove that the posterior columns are not the way of transmission of the sensitive impressions; but, although they have not this function, they appear to be very sensitive, and it results from a very curious physiological fact, which I have discovered, that they contain sensitive nerve-fibres, which appear to be directed quite in opposition to what might be imagined. They seem to go obliquely from the posterior columns downwards, instead of going towards the brain. See my paper on this subject in the Boston Med. and Surg. Journal, No. 16, 1852, and in the Gaz. Med. de Paris, 1853, p. 430.

† Anatomists now generally agree about these facts: 1st. That the decussation of fibres in the inferior and anterior part of the medulla oblongata takes place only between fibres of the lateral columns; 2d. That some of the fibres of these columns, as well as all the fibres of the anterior columns, do not decussate there; 3d. That the pyramids contain some fibres, received from the anterior columns, and which do not participate in their decussation.

voluntary motor nerve-fibres, according to what we know of the pathology of the brain, must decussate somewhere, we are led to admit one of the four following opinions, each of which appears to be supported by some facts :

1st. The decussation of *all* the voluntary motor nerve-fibres takes place in the medulla oblongata, where the fibres of the lateral columns cross each other to form the anterior pyramids.

2d. The decussation of the voluntary motor fibres takes place partly in the medulla oblongata, but for a greater part in the Pons Varolii, and in front of it.

3d. The decussation of the voluntary motor fibres takes place in the medulla oblongata and in the spinal cord.

4th. The decussation of the voluntary motor fibres takes place together in the spinal cord, the medulla oblongata, the Pons Varolii and the parts in front of it.

I cannot discuss at length here the value of each of these opinions, but I must say a few words about them. I will say first, that, after having examined the value of the pathological cases I have related, and of others, for which I have no room, it does not seem possible to doubt that there are very few (if any) voluntary motor fibres, which reach the Pons Varolii, without having made their decussation.

Therefore we must admit that the fibres of the anterior columns, and those fibres of the lateral columns, which do not decussate in the medulla oblongata to form the anterior pyramids, are not voluntary motor fibres, or that they have made their decussation in the spinal cord.

As I have said already that, in man, there appears to be no decussation of voluntary motor nerve-fibres, in the spinal cord, we are led to admit that the fibres of the anterior parts of the medulla oblongata (except those of the pyramids) are not voluntary motor fibres.

Now, there are facts, of which I have already spoken, which might prevent our admitting that the fibres of the anterior columns are not voluntary motor fibres; but we must say, about those facts, that if they are able to prove something for the fibres which are in the anterior columns, in the spinal cord, they prove nothing at all for the fibres which exist in the anterior columns, in the medulla oblongata. To have a decisive value, these facts should be accompanied with a demonstration of the continuity of the fibres of the anterior columns in the two

parts : spinal cord and medulla oblongata. But there is no such demonstration given, and, still more, here are facts which appear to prove that such a continuation does not exist, at least, for a certain number of fibres.

Exp. I.—If we make, at the level of the 10th costal vertebra, in a mammal, (dog, cat, rabbit, or Guinea pig) a transversal section of the two anterior columns of the spinal cord, there is a paralysis of movement more considerable, than if we have divided the two lateral columns. It seems, then, positive that, at this place, the anterior columns contain more voluntary motor fibres than the lateral columns.

Exp. II.—If we make, in the cervical region, the same comparative experiments, we find that the section of the lateral columns produces a more considerable paralysis, at least, on the posterior limbs, than the section of the anterior columns. The number of voluntary motor fibres appears, then, to have diminished in the anterior columns and to have increased in the lateral.

Exp. III.—If we make, above the decussation of the pyramids, a transversal section, on both sides, of the part of the medulla oblongata, which appears to be the continuation of the anterior columns of the spinal cord, we find that there is only a very slight paralysis produced. I would have liked to compare the result of this operation to the result of the section of the pyramids, which are the continuation of the lateral columns ; but I have not succeeded to divide them alone. Nevertheless we may make the comparison otherwise : if, after having made the section of the apparent continuation of the anterior columns, we divide the pyramids (and this may be done easily), we observe that the animal who, at first, was hardly paralyzed, becomes completely paralyzed.

This experiment appears to prove that the voluntary motor nerve-fibres exist, almost only, in the pyramids, and that there are but few of them in the apparent continuation of the anterior columns, in the medulla oblongata.

It results, also, from this experiment and from the two others, that the voluntary motor fibres are at first almost all in the lateral columns, in the medulla oblongata (in the pyramids), and that they leave gradually these columns to pass into the anterior, so that, at last, the lateral columns contain less motor fibres than the anterior.

Besides, according to researches made by Van Deen, Stilling, and myself, there appears to be in the anterior columns of the spinal cord, many nerve-fibres which are neither sensitive nor motor (at least motor of muscles of animal life). These fibres are inexcitable, like those of the brain and cerebellum, and I think that they may be considered as longitudinal commissural fibres.*

Now, to conclude, I will say that, from the four opinions, which I have related, as having facts in their support, the first appears to be somewhat too exclusive. It may be that some few fibres go up to the Pons Varolii, or still higher, to make their decussation, but there are strong reasons, nevertheless, which prevent my admitting that it is positively so. It may be also that some fibres make their decussation in the spinal cord itself, although it appears not to be so, according to pathological cases observed in man.† But if the crossing of the pyramids do not contain *all* the voluntary motor nerve-fibres of the trunk and limbs, the number of these fibres, which do not decussate there, is certainly small.

CONCLUSIONS.

From the facts, experimental and clinical, contained in the three paragraphs of the first part of this paper, I think I am entitled to draw the following conclusions:

1st. The sensitive nerve-fibres of the trunk and limbs appear to make the greatest part of their decussation, if not the whole of it, in the spinal cord, and not in the Isthmus of the Encephalon, as it was admitted.

2d. The voluntary motor nerve-fibres of the trunk and limbs appear to make their whole decussation, or, at least, the greatest part of it, in the inferior portion of the medulla oblongata, and not in the other parts of the Isthmus of the Encephalon, as it was admitted.

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* The recent researches of micrographers, so discordant in many respects, as regards the structure of the spinal cord, agree, nevertheless, in one point: it is that there are, in the anterior part of the spinal cord, transversal commissural fibres. The anterior columns, therefore, appear to contain transversal and longitudinal commissural fibres.

† See a Note of mine, on this subject, in the Comptes Rendus de la Soc. de Biologie. Vol. II., p. 195.

3d. According to the seat of an alteration in the cerebro-spinal axis, producing a paralysis, there are three different kinds of paralytic effects, which may exist:

a. The alteration being in any part of the Encephalon, except the inferior portion of the medulla oblongata,—the paralysis of voluntary movement and of sensibility exists in the side of the body opposite to the side of the disease.

b. The alteration occupying an entire lateral half of the inferior portion of the medulla oblongata, at the level of the decussation of the pyramids,—the paralysis of voluntary movement exists on both sides of the body, but incomplete, and the paralysis of sensibility exists only on one side, and it is the opposite to the side of the disease.

c. The alteration occupying the entire thickness of a portion of a lateral half of the spinal cord,—the parts of the body situated behind it, on the same side, are paralyzed of voluntary movement, and the corresponding parts, on the other side, are paralyzed of sensibility.

§ IV. *Paralysis of Sensibility in different parts of both sides of the body, produced by an alteration in only one side of the spinal cord.*

The paralytic symptoms produced by an alteration of a lateral half of the spinal cord, vary greatly according to its extent transversely and longitudinally. I do not intend to speak here of the differences which are due to the place of the alteration in a limited transversal part of the cord, i. e., either one of the white columns or the gray matter. My object now, is, merely, to examine the effects produced by an alteration of the entire thickness of a more or less extended portion of a lateral half of the cord.

To avoid complications and long explanations, in the preceding parts of this paper, I have thought better to mention only two of the paralytic effects, produced by an alteration of a lateral half of the spinal cord. Nevertheless, a third one exists, which consists in a loss of sensibility on the side of the alteration and in the parts of the body which derive their sensitive nerve-fibres from the altered part of the spinal cord. If the longitudinal extent of the alteration is only that of the insertion of the roots of one of the spinal nerves, the loss of sensibility, on the same side of the body, is very nearly limited to the parts receiving

their sensitive fibres from this nerve.†

There ought to be also a slight degree of anesthesia in parts of the body receiving their sensitive nerve-fibres from the posterior roots of the nerve or of the two nerves immediately below the alteration, on account of the fact that some fibres do not decussate at once and go upwards, a certain length, before decussating.

From what we have said it will be easy to deduce what must take place in all the cases in which alterations, able to produce paralysis, occupy the entire thickness of a part of a lateral half of the spinal cord. To render the subject clearer I think proper, nevertheless, to say a few words on three different kinds of paralysis, produced by an alteration of a lateral half of the cord and all characterized by the existence of paralysis of movement on one side of the body and a more or less extended paralysis of sensibility on the two sides of the body.

If an alteration, able to produce paralysis, exists in the whole thickness of a lateral half of the cord in the entire extent of the part from which come all the nerves going to one of the upper limbs, there will be paralysis both of movement and of sensibility in that limb, and paralysis of movement in the trunk and the inferior limb, in the same side of the body, and, besides, paralysis of sensibility in the opposite side of the body (limbs and trunk).

If an alteration, able to produce paralysis, exists in the whole thickness and length of the part of a lateral half of the cord, which gives all the nerves going to one of the inferior limbs, there will be paralysis both of movement and of sensibility in that limb and only paralysis of sensibility in the opposite limb.

If an alteration, able to produce paralysis, exists in the whole thickness and in the whole length of a lateral half of the cord, the symptoms will be a paralysis of movement, in the side of the body corresponding to the side altered in the cord, and a paralysis of sensibility in the two sides of the body (neck, trunk and the four limbs).

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† I have already said that most of the sensitive nerve-fibres *appear* to make their decussation, directly after their entrance in the spinal cord, as elements of the posterior roots. This is a mere appearance and there are facts proving, as I will show hereafter, that the fibres which penetrate in the spinal cord, in each of the posterior roots, behave there like the fibres of the sensitive root of the trigeminal nerve, in the Pons Varolii. A few go upwards, some almost transversely, towards the mesial plane, and many go downwards to make their decussation below their point of introduction in the nervous centre.

As the sensitive nerve-fibres, coming from one side of the body, have to pass through the corresponding side of the spinal cord to go to the opposite side of this organ, it is easy to understand that an alteration occupying the entire thickness of the cord is able to produce a paralysis of sensibility together with a paralysis of movement, in the same side of the body. But it must be remembered that if, in such cases, the paralysis of movement exists in all the parts of the body, receiving their nerves from the altered part of the cord and also from all the healthy parts below it (if there are any), the paralysis of sensibility, in the same side, remains limited almost entirely, to the parts which receive their nerves from the altered portion of the spinal cord.

To conclude I will say that any alteration of a lateral half of the cord, occupying its entire thickness, and able to produce paralysis, will cause :

1st. A paralysis of movement in the same side and a paralysis of sensibility in the opposite side of the body,—these two paralyses having the same extent;

2d. A paralysis of sensibility in the side of the body where the alteration exists and limited to the parts receiving their sensitive nerve-fibres from the roots of the spinal nerves penetrating the cord in, or very little below, the altered portion.

§ V. *On some pathological cases and other facts which appear to be opposed to the deductions that have been drawn from the facts and reasonings contained in the four preceding sections.*

The conclusions which are to be found at the end of the third section of the first part of this paper, imply that the cerebro-spinal axis is necessary for volition and for sensibility; they imply, also, that volition and the perception of sensations are performed in the Encephalon and not in the spinal cord; besides, they, also, imply that a decussation takes place for the sensitive and voluntary motor nerve-fibres, in some parts of the cerebro-spinal axis. But there are facts opposed to all these views, and, consequently, opposed to the conclusions I have arrived at. I will try, here, to answer to the objections which might be drawn from these facts, against these conclusions, and I hope that some light will be thrown by this discussion upon some of the most important problems relative to the physiology and pathology of the nervous system.

I will examine alternately the value of the following cases, experiments and anatomico-pathological dissections.

1st. Cases in which voluntary movements and sensibility are said to have existed in children apparently deprived of encephalon and spinal cord.

2d. Cases in which, although the spinal cord appeared to be destroyed in all its thickness, voluntary movements and sensibility are said to have existed, in the parts of the body, behind the destroyed parts of the cord.

3d. Cases and experiments which appear to prove that voluntary movements and sensibility have their centre in the Pons Varolii and the Medulla Oblongata.

4th. Cases in which a considerable alteration of Pons Varoli or of the Medulla Oblongata has not produced paralysis of movement or sensibility.

5th. Cases in which an alteration existing in the two sides of the Pons Varoli appeared to have produced a paralysis only in one side of the body.

6th. Cases in which an alteration in one side of the Pons Varoli or the neighboring parts appears to have produced paralysis in both sides of the body.

7th. Cases in which an alteration in one side of the Pons Varoli or neighboring parts has produced paralysis in the same side of the body.

8th. Cases and experiments proving that there are, in different parts of the Encephalon and in the spinal cord, motor nerve-fibres which are not voluntary motor.

9th. Anatomico-pathological dissections which appear to prove that nerve-fibres coming from the spinal cord decussate not only in the Medulla Oblongata but in parts above it.

Before discussing the value of these different facts, I ought to ask, from my readers, to examine and to weigh carefully my reasonings, and I indulge the hope that, if they do so, they will adopt my conclusions, however strange and opposed to heretofore received opinions, some of these conclusions may, at first, appear to be.

1. *Alleged voluntary movements and apparent existence of sensibility in children apparently deprived of the cerebro-spinal axis.*

I think that it is now impossible to deny that there has been cases in which such monsters (*Amyelencephalous*, Béclard,) have had move-

ments, either in or out of the uterus, and without doubt purely reflex and which have been mistaken for voluntary movements and admitted as a proof that sensibility was existing. Among numerous cases collected by Isidore Geoffroy St. Hilaire* and by Ollivier d'Angers† there are some which cannot be considered as entirely erroneous statements and, to laugh at the believers in these facts, as Longet‡ does, cannot be an argument against their existence. What is to be done is, no more to deny, but to try to explain, and this, I will do, after having related some extremely curious cases, recently published and, perhaps, better authenticated than those of which Longet has spoken.

The first case I will relate is recorded in the very rich and interesting catalogue of the Boston Anatomical Museum, for which science is indebted to the zeal and activity of Prof. J. B. S. Jackson.

Case. I.—A pregnant woman did not feel the movements of her child, until about the end of the fifth month and they were always feeble and peculiar. In the last month slight motions of the child were still occasionally felt, even after a profuse discharge of liquor amnii. Labor came on and was accomplished with very little pain; the child was born *alive*, the mother having felt its motions for fifteen or twenty minutes after it was expelled. On the arrival of Dr. Hildreth, half an hour afterwards, the lower extremities of the child, were still in the vagina.

The spinal marrow was wanting and, the spinal column being open throughout, the nerves terminated in the membrane upon its posterior face. A very small bundle of nervous fibres was seen passing down over five or six of the processes upon the left side, and a few of the cranial nerves were found, among which, it was thought, was the par vagum. Nerves of the trunk and extremities well developed.

The brain existed, weighing three ounces (the weight of the monster being three pounds and one ounce). It rested upon the expanded dorsal and lumbar vertebræ and upon the integument which covered the depressed cranial bones. It was divided into two equal hemispheres and, imperfectly, into convolutions, the arachnoid membrane being continuous about the base with the common integument. The whole mass was very soft, and of a dusky color, from congestion, and effused blood; there being in each hemisphere a cavity which was filled with

* Hist. des Anomalies de l'Organisation, Vol. II. 1836.

† Loco cit. Vol. I, p. 146.

‡ Anat. et Physiol. du Syst. Nerv. 1842, Vol. I, p. 323.

coagula. No other parts of the brain were recognized, and no connection was traced between this mass and any of the nerves, either cerebral or spinal.*

It is to be regretted that some circumstances have not been pointed out, in this case, and, for instance, the size of the ganglions of the sympathetic and of the spinal nerves.

The movements of the child, after birth, not having been ascertained by the physician himself, we cannot know whether they were respiratory, convulsive or reflex. As to what took place before birth, if we can rely upon the assertion of the mother there has been movements, but they began late, although much sooner than these uterine contractions which are sometimes mistaken for movements of the child. The mother had had already two children and, therefore was able to distinguish the movements of a child.

I must say also that, in a physiological point of view, although the brain was existing in this monster, it is exactly as if it had been missing because it was not connected with the nerves.

In the following case we will find that movements have existed, during pregnancy, in a case, the circumstances of which, may lead us to explain how movements may exist.

Case II.—A woman, pregnant for the sixth time, was not aware of the fact, although her abdomen had become large and her menstruation had stopped, and she was much surprised when she felt the movements of her child, which were so strong that, even the hand of another person could perceive them. The 8th of May, she felt the movements for the last time and after the expulsion of a very considerable quantity of water, she was delivered of a child, who, out of the uterus, remained motionless. Its weight was from 3 to 3½ lbs. It had no neck and the head, very small, appeared to be lodged in a cavity in the trunk. From the upper part of the orbitary arcads the skin was missing all over the head. The spinal canal, open all along, from the cranium to the lumbar region, contained a spinal cord, being in shape like a band, flattened from behind forwards and of about one line and a half in thickness; its beginning at the base of the cranium was somewhat bifurcated and evenly cut. It was lying on a fatty and fibrous

* A Descriptive catalogue of the Anatomical Museum of the Boston Society for Medical Improvement. By J. B. S. Jackson. 1847, p. 263. (Case by Dr. Charles T. Hildreth.)

mass, which filled a deep excavation, found in the place where the neck should have been, had it existed. This medullary band is very loosely attached to the spinal canal, by some fibrous filaments. This band had no connection with the nerves, which terminated in the vertebral foramina by a swollen and ganglionary extremity. In the trunk and limbs, the nerves were well developed. There was no brain.

The chest was large; no respiration had taken place, as the lungs showed. The upper part of the cranium was missing and the basis was convex.

It is very much to be regretted that Dr. Olier,* to whom we owe the relation of this interesting case, has not given more details about the nervous system. He does not speak either of the cranial nerves or of the sympathetic.

I have now to examine the value of the two cases I have related and of many other analogous to them, which are more or less authenticated.

The first question is whether there has been any decided voluntary movement and any perception of a sensitive impression in these monsters.

Nothing proves that sensibility exists, and the movements consecutive to an excitation, however regular or co-ordinate they may be, cannot give such a proof. Nothing, in these circumstances proves that these movements are not reflex. It is now generally admitted, as Bischoff,† Dr. Simpson‡ and others have established, that the movements of healthy children in the uterus, and even for sometime after birth, are merely reflex. If it is so for them, *a fortiori*, is it so for the *Amyelencephalous* or *Pseudencephale* monsters. These reasons against the existence of sensibility may be employed against that of a Will, in these monsters. Their movements are almost always consecutive to an external excitation. Nevertheless we admit that they have, sometimes, an *apparently* spontaneous movement; but, we well know that reflex movements may be excited by any irritation of the viscera and that the respiratory movements of the chest may excite reflex movements, in the limbs. Besides, the same excitation which produces respiratory

* Observ. d'un fœtus Anencéphale, in Comptes Rendus de la Soc. de Biologie. 1850, Vol. II, p. 106.

† Traité du dévelopment de l'homme.—Trad^on. françoise, 1843, p. 459.

‡ Edinb. Monthly Journal of Med. Science, July 1849.

movements, is able to produce movements in the limbs. I have tried to prove in a book*, which I published two years ago, that the nerves, muscles and some other parts of the body, may be excited to act, by blood, containing a great quantity of carbonic acid. Very likely, respiratory movements, in monsters, as well as in healthy children and adults, take place in consequence of an excitation produced by carbonic acid, and an excitation, by this agent, may also be the cause of some of the falsely called voluntary movements in the Pseudencephalic monsters.†

To conclude we will say that, if there appears to be no doubt that movements may exist in these monsters, nothing proves that they are voluntary and that they result from true sensations. On the contrary they appear to be purely reflex or mere excited movements.

A second question now arises, much more difficult to be solved than the preceding. How to explain reflex movements, if there is no spinal cord or if this organ, though existing, is not united with the nerves?

Let us examine alternately the different possible explanations.

1st. A man of genius, Etienne Geoffroy St. Hilaire‡ has given credit to the idea that the liquid which is sometimes found in the Pseudencephalic monsters, in the cranio-spinal cavity, and filling there a tube, formed by the meninges,—may be considered as containing the elements of the brain and spinal cord, in their primitive state of development. In many cases this liquid is not found in the membranous tube formed by the meninges, but then, it appears to have gone out, together with the liquor amnii, before parturition.

This theory is quite opposed to the well known doctrine of Morgagni,§ who admitted that the production of the liquid, found in, or coming from the cranio-vertebral cavity, is the result of a morbid ac-

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* Experimental researches applied to Physiol. and Pathol. New York, 1853, p. 101-113.

† The Pseudencephalic monsters compose the Genus *Pseudencéphale* of the family of the *Pseudencéphaliens* of E. G. St. Hiliare. They correspond to the *Amyélencéphales* of Béclard. The characters of the Genus are to have the spinal canal open and no spinal cord, and, besides (as family characters), no true encephalon and, in its place, a vascular tumour, which may contain some nervous matter.

‡ Philosophie anatomique.—Des Monstruosités humaines, 1822, Vol. I, p. 125-153.

§ De sedibus et causis Morborum. Epist. 12.

tion, and that a true dropsy is the cause of the destruction of the nervous centres.

Of these two opinions, if we adopt the first, as regards the source and the nature of the liquid, we cannot, nevertheless, adopt entirely the view of E. G. St. Hilaire, that this liquid may act as the brain and spinal cord would. We know that as long as the nervous centres are in a liquid state, in the human embryo, there are no movements, and I have ascertained that this does not result from an insufficiency in the development of muscles. In embryos of rabbits, from eight to twelve days old, I have found the muscles irritable and producing movements when directly excited, and their nervous centres, then almost entirely liquid, appeared not to be endowed with any power of action. But it may be that the liquid we find in the vertebral canal of monsters, contains a sufficient number of soft nerve-fibres and cells, to produce some reflex movements. Micrographers, and they alone, may solve the question of the existence of these nervous elements in this liquid.

If we admit as true the doctrine of Morgagni, it belongs still more to micrographers to point out what nervous elements are left in the cases where the naked eye shows only a liquid.

2d. A second explanation might prove good for a number of cases, and especially those like the one of Dr. Olier, which I have related. A rudiment of the spinal cord there exists, but is not found united with the nerves. For who knows, how easily in fœtuses, healthy or not, the roots of the spinal nerves, may become separated from the spinal cord and also from the ganglions and, besides, may break in any part of their length, particularly when there is an unusual amount of water around them,—it will be easy to understand that, in Monsters, the roots of the nerves may not have not been found, although they may have existed. It is known that a very able Anatomist, Desmoulins, having vainly searched for the roots of the spinal nerves, in fishes, concluded that they do not exist and that the nerves, in these animals, are attached to the meninges. Desmoulins has been greatly mistaken, and if he has been so, in such circumstances, it will easily be admitted that others, not so accustomed as he was to dissections, may commit the same error, in circumstances more favorable to a mistake.

In experiments, that I have performed on Birds and other animals, I have found that reflex actions may exist after considerable alterations of the spinal cord. I will hereafter give the details of some of these facts and of some others proving that, not only, then, reflex actions

may exist, but also sensations and voluntary movements. We can, therefore, understand that a small stripe of the spinal cord, as in the case of Dr. Olier, if it is connected with the nerves, may be the cause of the reflex movements, existing in the Pseudencephalic.

3d. We are led to propose a third explanation, based upon some anatomical facts, observed in Amyelencephalians, and also upon the experiments we have just spoken of, and which prove that small parts or stripes of the spinal cord may be sufficient for reflex actions.

In some cases of Anencephalia or of Pseudencephalia, the roots of the nerves have been found hanging, apparently, as loose threads in the cranial and spinal cavity. In cases of this kind, reported by Dr. Lonsdale* and Prof. Paget,† it has been found that the nerve-fibres formed loops, imbedded in a filamentous tissue and surrounded by numerous granules. It is probable, and it is considered to be so by Prof. Paget, that this granular matter may be regarded as gray matter, in an early stage of development.

I am not prepared to say that these loops and this slightly developed gray matter are capable of producing reflex movements, strong enough to be observed, but I think that they may assist other parts in the production of these movements.

An amount of gray matter, much greater than that found by Drs. Lonsdale and Paget, may certainly have existed, in cases of *Amyelencephalia*, and no notice taken of it, by Physicians who were not prepared to consider it as nervous matter. And this amount of gray matter, connected with the roots of the nerves, may have been able to produce reflex movements.‡

The possibility of existence of reflex movements when the spinal cord is reduced to only a part of its substance, is well proved by the following experiments. Two pigeons had had their spinal cord divided

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* Edinb. Med. and Surg. Journal, January '44.

† Brit. and For. Med. Rev. No. 48, p. 273.

‡ It is very much to be regretted that the recorders of the cases numbered 776, 778, 781, in the *Catalogue of the Boston Museum*, have not said if the mothers of the monsters they describe, had felt them move, during the last days of pregnancy. As regards the case No. 776, p. 254, the writer says: "A trace of brain only existed, if any at all, but in the situation of the pituitary gland, there was found a soft, rounded, reddish mass; and there was some appearance, also, of a thin and superficial layer of medullary substance in place of the spinal marrow; and the same has been sometimes observed in other similar cases; spinal nerves and ganglia well developed, as they usually are in these cases."

transversely, at about the middle of its length. A metallic wire was passed in the spinal canal and pushed, from the place where the cord had been cut, until the level of the second or third caudal vertebra. In one of these animals, there has been, a short time after the operation, weak but positive reflex movements, in the tail and the posterior limbs, whatever was the excited point. For many days and until the time when this animal was killed, reflex action lasted. The dissection of the animal was made, at a meeting of the Society of Biology,* and all the posterior surface of the cord was found covered with coagulated blood; the cord was flattened from behind forwards, softened in all its thickness, and it had, in many places a violet, reddish color, without doubt, due to infiltrations of blood. This half liquid gray matter which, in birds, exists in the rhomboïdal ventricle, was destroyed,† and the posterior columns of the cord were, almost everywhere, separated one from the other, so that the central gray matter was laid bare.

In the other pigeon there were evident, but weak, reflex movements, only in the left posterior limb and in the tail; excitations of the skin of the right posterior limb were absolutely without effect. Dissection being made, in presence of the Society of Biology, it was found that, in a great length, the right side of the spinal cord had been entirely destroyed, and that the other half, which existed, was red and softened, and in communication with the roots of the nerves and with the caudal part of the cord, which was but little altered.

I have performed other experiments of the same kind, on some mammals, and succeeded in obtaining like results, particularly on very young ones. Newly-born cats are the best animals for such experiments. Before my researches, experiments of this kind, had been made, but only on frogs and other cold-blooded animals, by Volkmann, Van Deen and Stilling.

The resemblance between what takes place in the animals, I have experimented upon, and what exists in monsters, extends farther than the coexistence in both, of reflex movements and of a small amount of nervous matter, in the spinal canal. I have found‡ that, in animals, having had a part of their spinal cord crushed, there were, besides re-

* See Comptes Rendus, Vol. II, 1850, p. 47.

† I have already published, five years ago, a very curious fact about this gray matter: it is that when it is taken away, a reproduction of a similar substance takes sometimes place in a very short time.

‡ See Comptes Rendus de la Soc. de Biologie, Vol. III, 1851, p. 15.

flex movements, convulsions, and, in certain muscles, contractures producing deviations of different parts of the limbs. So that, I had there, under my eyes, the phenomena which exist in the production of deviations, in the limbs of monsters, according to the theory of Dr. Jules Guérin.

4th. The last explanation I will propose, is founded on the fact that the ganglia of the spinal nerves and those of the sympathetic are generally much developed, in monsters deprived of a great part or of the whole of their cerebro-spinal axis, as Breschet, Lallemand‡ and others have pointed out.

In the case of Dr. Olier, above reported, we see that the spinal nerves had, in the vertebral foramina, a swollen and ganglionary extremity. In a very interesting case of Acephalia, recorded by Prof. O. W. Holmes,§ although the spinal marrow existed, the ganglia of the sympathetic nerve, and the filaments connecting them, were unusually developed in the thorax, abdomen and pelvis; in the thorax were two ganglia, which extended from the upper rib to about the eighth rib upon the right side and to the sixth on the left. In this case the two upper limbs and the head were missing.

The increase in size of the ganglia in these monsters, may easily be explained. The blood, containing the chemical elements, which are to be used in the formation of the nervous tissues, their deposition ought to take place, in the only parts of the nervous system which exist, and, the less there are of such parts, the greater, as a general rule, ought to be the accumulation of nervous matter in them.

It is to be regretted that in cases where the spinal cord was missing, and the ganglia of the sympathetic nerve were enlarged, it has not been examined if the branches of communication between the motor and sensitive spinal nerves, were not larger than usual. The importance of such an examination becomes evident if we suppose a case of *Amyelencephalia*, in which it would be positive: 1st. that there has been recently more or less reflex movements; 2d. that there is no trace whatever of the encephalon and of the spinal cord.

Then, we have either to give up all the teachings of daily experimentation and observation, about the necessity of nervous centres for the production of movements, after an excitation of the skin or of another surface,—or to look upon the ganglia of the spinal nerves or of the sympathetic, or of both, as the nervous centres which, by their

‡ Observ. pathol. propres à éclairer plusiers points de Physiol. 1818, p. 30.

§ Catalogue Boston Museum, p. 245.

reaction, originate these movements. At first, we already know that these ganglia are larger than usual; but to admit the hypothesis to which we are forcibly led, some other things must exist. If the ganglia of the sympathetic alone are enlarged and are endowed with reflex power over the muscles of the limbs and trunk, of course, the nerves of these parts must be connected by many more nerve-tubes than usual, with these ganglia. In other words the origin of the excito and reflecto-motor nerve-fibres of the limbs and trunk must be in these ganglia, instead of being, as normally, in the spinal cord. This implies, not only that the branches of communication, between the spinal nerves and the ganglia of the sympathetic, must be larger than usual, but, also, that the fibres come from the periphery of the spinal nerves into the branches of communication. If, instead of being in the ganglia of the sympathetic, the enlargement is in those of the spinal nerves, then the fibres of the motor roots, instead of merely passing along the ganglia, will penetrate them, and be connected there with the cells of the gray matter and with the excito-motor fibres, as, normally, in the spinal cord.

The four explanations or rather the four hypotheses, which I have exposed and by which, I believe, that we may understand how are produced the reflex movements, in Monsters, apparently deprived of the cerebro-spinal axis, may be solved by future examination. My object in relating these hypotheses was double; it was, 1st to show, that there is possibility of explaining the movements of these monsters, so that we are not compelled to admit that our general ideas, about the physiology of the nervous system, are not grounded; 2d, to call the attention of future observers to what they will have to examine in *Amyelencephalous* monsters, and to the causes of fallacies, in making such an examination. And, in view of this second object, I will sum up, here, what, I think, ought to be searched in such cases.

1st. The composition, chemical and microscopical, of the liquid, contained in or coming from the tube formed by the meninges, to ascertain the degree of analogy, between this liquid and the nervous tissues, in an early period of development.

2d. The existence or absence of small masses of granular matter, connected one with another and connected, also, with the spinal and cranial nerves. Of course the nature of the granular matter must also be determined.

3d. The existence of communications between the nerves and a more or less developed stripe of spinal cord; and, about this, it must be re-

membered that the nerves are so soft, so fragile that they may be very easily separated from the spinal cord, as it may have been, in one of the two monsters the case of which is above described.

4th. The enlargements of the ganglia of the spinal nerves and of those of the sympathetic and all what relates to the communication of these ganglia with the nerve-fibres coming from or going to the trunk and limbs, must be examined carefully.

There are some important facts relative to the Pseudencephalic monsters, of which I must say here a few words.

As far as examination with the naked eye goes, it seems that the development of muscles and nerves in these monsters, born at full term, is, almost always, as perfect as in the most healthy newly-born children.* The fact that muscles are well developed, proves that they have been put in action, and as they can, hardly, have been acting, without having been excited by nerves, it follows that the nerves must have been able to act, at least, until a short time before the monster is expelled from the uterus. And as the nerves do not act spontaneously, but after an excitation, and, besides, as there is no known sufficient cause of direct excitation of the reflecto-motor nerves, it results that the action of these nerves is a secondary one, produced by a reflex action from a nervous centre, itself excited, in consequence of an irritation of some excito-motor nerves, in the skin or in the mucous membranes.

This series of deductions being right, it may fairly be concluded that even in cases where it is not known if a mother has felt the movements of her child, monstrous or not, it is probable that it has had reflex movements, if its muscles are well developed. In cases of monsters, in which the nerves have not been able to act on the muscles, either because there are no nervous centres at all, able to put them in action, by their reflex power, or because the nerves have not had their natural development, the muscles do not develop themselves, or if they have had a beginning of development, they become atrophied and changed into fibrous or fatty

* The theory of Tiedemann (Zeitschrift fuer Physiol., Vol. I, p. 56, Vol. III, p. 1,) that the nervous system is necessary to the development of the embryo is certainly not more exact than the analogous theory, according to which the nervous system is necessary to secretion and nutrition, in children and adults. As I have tried to show elsewhere (Exp. researches applied to Physiol. and Pathol. New York, 1853, p. 6-17), the numerous facts, which establish that the nervous system may have a great influence on nutrition and secretions, do not prove that this influence is necessary and there are many other facts proving, on the contrary, that these functions may exist, without any intervention of the nervous system. Against the views of Tiedemann, see *Bischoff, loco cit.* p. 468.

tissue, according to circumstances, which an eminent physician, Dr. Jules Guérin, has pointed out.

Alessandrini,* has described two monsters, in which the inferior part of the spinal cord and its nerves were insufficiently developed, and the muscles of the posterior limbs were missing, although the blood-vessels, bones, and other parts, partly existed. In cases of absence, not only of the nervous centres, but also of the nerves, such as have been recorded by Clarke† and by Hempel,‡ the muscles had hardly any existence.

As a conclusion to all that we have exposed about the Amyelencephalous or Pseudencephalic monsters, we will say that their movements appear to be merely reflex and that, although they are deprived of the greatest part of their nervous centres, there is, according to the greatest probability, enough of these parts remaining to produce reflex actions.

Therefore, it cannot be concluded, from the facts observed in monsters, that the encephalon is not necessary for the perception of sensitive impressions and for the production of voluntary movements.

2. *Alleged voluntary movements and apparent existence of sensibility, in parts of the body, considered as deprived of their natural connection with the encephalon.*

As I propose to treat this subject at length, in a special paper, I will merely say here: 1st. That the cases of softening of the spinal cord, related by Velpeau,§ Ollivier d'Angers and Abercrombie, in which no paralysis has existed, although there appeared to be an interruption of continuity, between the nerve-fibres of some parts of the body and the encephalon, are not fit to prove that this continuity is not necessary. Until it is demonstrated that the nerve-fibres and the cells of the cord lose their properties when they are softened, these facts will prove nothing against the necessity of the continuity of nerve-fibres, for sensation and for volition. But, it will be said that there are cases, where the softening was so considerable that the membranes of the cord contained only a liquid, which flowed out, when the membranes were opened. This also has no value: where is the proof that it was so during life? Is there any physician, knowing the facts,

* Novi Comment Scienc. Instit. Bonon, Vol. III, 1837.

† Philos. Trans. 1793.

‡ De monstris Acephalis, Hafniœ, 1850, p. 38 and Tabulœ 5 and 6.

§ Archives gén. de Médec. Vol. VII.—1825.

published by Calmeil,‖ and which demonstrate the extremely great rapidity of production of softening, which may take place in the spinal cord, a short time before or after death, and who will maintain that the liquefaction of the cord had existed previous to death, without causing paralysis?

2d. As to the cases of induration of the spinal cord, the same thing may be said as for cases of softening. Until it is shown that in cases of induration, in which voluntary movements and sensibility have continued to exist, there was an impossibility for the spinal cord to perform its functions, these cases will merely prove that the functions of this organ, may remain, although its organization appears to be much changed.

3d. We will show elsewhere that the same reasoning may be applied to the cases in which the transmission of nervous action from or towards the brain apparently continued to take place, in the spinal cord, although it was much atrophied, in consequence of a compression or any other cause.

4th. There are cases in which it is said that the spinal cord had been, partly or entirely, cut across, and in which there was no paralysis of sensibility or voluntary movement. But in these cases, the section had taken place near the lower extremity of the spinal cord, as in the case so often spoken of, recorded by Desault,‡ so that almost all the nerves had their origin above the section. Besides, when a sword has been introduced into the spinal cord, as in a curious case, related by Ollivier d'Angers§ we may understand that few of the fibres of the cord had been cut by the thin extremity of the sword.

We, therefore, will conclude that the facts of softening, induration, atrophy or division of the spinal cord, cannot prove against the necessity of a communication of the encephalon with the different parts of the body, through the spinal cord, for the existence of voluntary movements and sensibility.

3. *Alleged persistance of sensibility and voluntary movements in men and animals, deprived of all the parts of the encephalon, except the medulla oblongata and the Pons Varolii.*

The facts upon which Physiologists and Pathologists have grounded their opinion that the upper parts of the encephalon are not necessary

‖ Journal des Progrès des Sciences Médic. 1828, Vol. XII, p. 172.
‡ Journal de Chirurgie. Vol. IV. p. 137.
§ Loco cit. Vol. I. p. 354.

for volition and for the perception of sensations are of two kinds: some are experimental, some clinical.

As to the experimental facts, they consist in showing that after the removal of the encephalon, except the Pons Varolii and Medulla Oblongata, the animals manifest that they feel pain by their cries and agitation. When, says Longet,* rabbits and dogs have been submitted to this mutilation, upon their encephalon, although they seem to be in a deep coma, they are still able to agitate themselves and to cry plaintively, under the influence of strong external irritations; but, if a sufficiently deep alteration is made, in the Pons Varolii, there is an immediate cessation of the cries and of the agitation; it merely remains an animal, in whom circulation, respiration and other nutritive functions are momentarily accomplished.

Longet concludes, from his experiments, that the Pons Varolii is the *seat of general sensibility* (faculty of feeling pain, etc.,) and *the centre for the perception of tactile impressions.*† Of these two deductions the second has no foundation whatever; there is not a single fact which may even appear to lead to it. As to the first deduction, if Lorry, Magendie, Desmoulins, Bouillaud, Gerdy, Serres, J. Mueller, have concluded, as Longet did, after them, that the existence of cries and of agitation, in animals, deprived of all their encephalon, except the Pons Varolii and the Medulla Oblongata, proves that the general sensibility then exists,—this merely shows that these eminent physiologists did not know how powerful may be the reflex power.‡ Of the two facts given as proofs of a perception of sensation, *i. e.* agitation and cries, the first, certainly, cannot prove that there has been a perception of pain. It is exactly the same thing that we see in limbs, connected by their nerves, with a part of the spinal cord, separated from the encephalon. If, in this case, we call the agitation a reflex action, why shall we give to it another name, in the case where the the spinal cord is connected with the Medulla Oblongata and the Pons? The agitation, it will be said, is greater in this last case, but the

* Traité de Physiol. 1850, Vol. II, B. p. 38.

† These are his own words, loco cit. p. 36 to 41.

‡ It may seem strange that I declare that Prof. J. Mueller, who has in common with Marshall Hall, the glory of having called the attention of Physiologists and Physicians, to the phenomena of reflex action, does not, nevertheless, know, how great may be, the power of the reflex faculty. But, to prove that I am right, I think it is sufficient to say that Prof. Mueller, at the time he published his view on the Pons Varolii, admitted that reflex action is greater in cold blooded than in warm blooded animals, and also that he was not aware how much pure reflex movements may be harmonious and well co-ordinated.

extent of the nervous centre, able to produce reflex movements, is greater, so that these movements must be stronger and more extensive.

Now, as to the cries, we may also consider them as mere results of reflex contractions, taking place in certain muscles. The vocal cords, becoming fixedly stretched, and the expiratory muscles contracting strongly and suddenly, the column of air expelled from the chest, passes along the stretched vocal cords and vibrations take place, producing the sounds we call cries. I have seen two hysteric patients who cried suddenly (although they tried not to do it), after the slightest irritation of the skin or after any kind of emotion.‡ It seems, then, that cries may be considered as mere results of reflex contractions. I must, nevertheless, say that I do not think that the question is positively decided. My only object now is to show that cries cannot prove that sensibility exists, because we may explain them, without admitting that pain has been truly felt.

All the writers on this subject, already named, had considered the Pons Varolii as being the organ in which the perception of sensitive impressions take place, and they agree in saying that, after the removal of the Pons, there are no more cries. They have been mistaken about this last fact, and I have obtained quite a different result, in performing this experiment on cats, rabbits and Guinea-pigs. After having removed, by slices, the whole encephalon, except only the Medulla Oblongata, I have observed that the animal, when pinched, is much agitated, and that it cries plaintively. Then, if the Medulla Oblongata is removed, there are no more cries, when the animal is pinched, but

‡ Dr. R. B. Todd has given some good reasons to show that the Pons Varolii is the centre for emotional movements (Cyclop. of Anat. and Physiol. Vol. III, p. 722, 2.) I am ready to admit that the Pons Varolii, particularly by its part connected with the roots of the auditive nerve, is a portion of the centre of emotional movements, but not the seat of the whole of this centre. The Medulla Oblongata, I think, is also a part of this centre. When a violent and sudden emotion causes death, it is in acting on the Medulla Oblongata, that it has such a powerful effect. An excitation is then produced on the roots of the *par vagum*, which appear to have their true origin in the neighborhood of the nib of the *Calamus Scriptorius*, and in consequence, the blood-vessels of the heart contract and expel the blood they contained and with it, the natural excitant which causes the movements of the heart. So that a complete syncope and death take place. It is in acting on the branches of the par vagum, in the lungs, as I will prove elsewhere, or on the Medulla Oblongata, that chloroform, sometimes, kill suddenly. The stopping of the heart's action in the celebrated experiment of the brothers Weber, take place in the same way, as I have tried to prove in my *Exp. researches*, p. 77, and p. 101 to 124.

the agitation continues.* There is great appearance that, after this mutilation, cries would exist if the motor nerves of the larynx, instead of being connected with the Medulla Oblongata, were connected with the spinal cord. Of the two things necessary for cries, one exists: the animal, being pinched, not only has agitation of the limbs and trunk, but a contraction of the expiratory muscles. Flourens† had already seen that there may be a respiratory movement, in such cases, and he says that a true inspiration, producing a sound in the larynx, exists when the animal is pinched.

Now we will conclude that, if cries prove that there is perception of pain, we must admit, in opposition to all the Physiologists already named, that the Medulla Oblongata is a centre for that kind of perception. Besides, the view of these Physiologists, as regards the Pons Varolii, is erroneous. Still more, if agitation proves that sensibility exists, the spinal cord possesses this faculty, as well as the encephalon. But we repeat that the reflex theory explains the existence of agitation and of cries, and we are not therefore compelled to admit that the spinal cord and the Medulla Oblongata possess sensibility.

The *will*, or, at least, the faculty, under the influence of the will, and by which the so-called voluntary movements are produced, is considered, also, by Gerdy, Mueller, Longet and others, as having its organ in the Pons Varolii and in the Brain. The reasons given by these writers to prove their views are far from being satisfactory. They prove merely that there are energetic movements after an excitation of the animal, and these appear, then, to be mere reflex movements. Longet himself (loco cit. p. 39) says, that if the cerebral hemispheres have been removed, and also the corpora striata, in rabbits, they can stand up or move forwards, but that after the removal of the thalami optici, although the Pons Varolii is left entire, there is no more possibility for the animal to walk or to stand up.

I have many times repeated these experiments upon rabbits and Guinea-pigs, and uniformly obtained nearly the same results, which, certainly, are a deadly blow to the theory advocated by Longet. Not only standing and walking become impossible after the removal of the whole encephalon, except the Pons and the Medulla Oblongata, but it

* These experiments I had already published, in the Comptes rendus de l'Acad. des Sciences. 1849. Vol. xxix. p. 672.

† Rech. expér. sur les Propr. et les Fonct. du syst. Nerveux. 2d ed. 1842, p. 178.

seems, also, that the apparently spontaneous movements which sometimes exist there, are mere convulsive movements.

Besides, I have found that the reflex movements, in this case, are only a little stronger than in animals deprived of the Pons Varolii. As to the regularity, the harmony, the direction of the reflex movements, they are alike in both cases.

The celebrated doctrine of Flourens, against which Bouillaud, Gerdy, Longet and others had proposed the theory, which I have just proved to be erroneous, does not appear to be right. Flourens has gone too far, I believe, in thinking that the faculty of perception for all the sensitive and the sensorial impressions, exists only in the cerebral lobes, and that the same parts are also the only seat of Intelligence and Volition. We will not discuss these questions here, but, we will say, as regards the perception of sensitive impressions and Volition, that experiments on animals, as well as pathological facts, observed in man, and also microscopic anatomy, agree in showing that the thalami optici and the corpora striata, and also the crura cerebri, appear to be the centres for these actions. I am glad to agree almost entirely with Dr. Todd, as regards the corpora striata, which he considers as the principal centres for voluntary movements. But I do not agree with him as to the thalami optici, which, according to his views, are the centres for almost all the sensitive and sensorial nerves. Anatomy and Pathology are opposed to this view, as I will try to show, in a special paper.

There are, on record, many cases of Acephalia or of Anencephalia, which might be considered as proving that sensibility and voluntary movements may exist, when the whole encephalon is missing, except the medulla oblongata alone or with the Pons Varolii. In all the cases of this kind which I have collected, I have found no evidence that the movements, which existed, were voluntary and not simply reflex or convulsive. I will give here a short analysis of two, among these cases, which appear to be the most favorable to the theory I reject.

Case I.—I have seen, says Lallemand,* an encephalous fœtus, born at full time, and which lived three days. During all this time, it cried with a certain degree of strength, and tried to suck whenever anything was put between its lips. It made movements, to some extent, with its legs and arms. When anything was put in its hands, there was a flexion of its fingers, as though it would seize it, but, generally, all its

* Loco cit., p. 52.

movements were less energetic than those of a healthy fœtus of the same age.

The cerebrum and the cerebellum were entirely missing; there were, only, on the basis of the cranium, the Medulla Oblongata and the Pons Varolii, with the origin of the pneumo-gastric, trigeminal and optic nerves.*

Although Lallemand concludes from this fact that sensibility and voluntary movements existed, in this monster, no one, acquainted with reflex action, will accept these deductions.

Case II.—A fœtus, nine months old, was born perfectly well developed, except that it was anencephalous. Its eyes were constantly shut; it frequently uttered cries, that ceased when a finger was put in its mouth; it, then, sucked repeatedly.† Its limbs were agitated, with some strength, and it pressed with its fingers the things that were placed in its hands. Three hours after birth, the number of respirations, in a given time, was diminished, and the cries were less frequent and less strong. Respiration diminished gradually and became convulsive. This state lasted for six or eight hours, during which, the cries became weaker and weaker and less frequent as, also, the respiratory movements which were accompanied with general convulsions, and, at last, it died in a true state of asphyxia. The cerebrum and the cerebellum were entirely missing, and of the basis of the encephalon, nothing existed, except a very irregular medulla oblongata, connected with a kind of Pons Varolii, merely consisting in a square layer of grey matter only two lines and a half long and two lines broad. The membranes of the spinal cord were inflamed.

This case, recorded by Ollivier d'Angers,‡ and considered, by him,

* Lallemand, in this description, has not shown the accuracy which, ordinarily, characterizes his publications. He says, that the optic nerves had their origin in the parts that remained of the encephalon; this implies, certainly, that some parts, at least, of the tubercula quadrigemina were also existing, although he does not mention it.

† Sucking, in Anencephalic monsters, may take place, as it takes place, also, in animals, deprived of almost all their encephalon. This fact has been well proved by the experiments of Grainger, (Observ. on the struct. and funct. of the spinal cord. 1837, p. 80,) of J. Reid, (Physiol. Anat. and Pathol. researches. 1848, p. 183,) and by my own, (Exp. res. applied to Physiol., etc. 1853, p. 5.) Grainger has also well proved that sucking is a mere reflex action, by pointing out what takes place in the fœtus of the opossum, in which sensation and volition cannot exist, and the lips of which remain attached to the nipple by contraction and grasping.

‡ Loco cit., Vol. I. p. 179.

as proving that the spinal cord and the Medulla Oblongata possess the two faculties of perception of sensitive impressions and of volition, certainly cannot prove such things. Had Ollivier known reflex action, he would have considered this case quite in another light. But there is something important proved by this fact, and which is entirely opposed to the view of Gerdy, Mueller, Longet and others : it is that the Pons Varolii was so slightly developed, that we can consider it as missing, and, nevertheless, agitation and cries have existed as when the Pons exist. If these phenomena prove that sensibility and volition exist, we must then admit that the Pons Varolii and the cerebrum are not the only centres for volition and for perception of sensitive impressions.

To conclude, we will say that both monsters and animals experimented upon, may cry and have movements, when they are deprived of almost all their encephalon (the medulla oblongata alone remaining), but that nothing proves that these movements and these cries are not mere reflex actions.

In other words, the facts, which have been considered, by almost all the French Physiologists of our day, as proving that the Pons Varolii is a centre for volition and sensibility, cannot prove such a thing. Besides, it is clear, also, that these facts cannot prove anything against the theory that, in men and animals, having their nervous centres well developed, transmission, for both sensations and volitions, has to take place through the Medulla Oblongata and the Pons Varolii.

4. *On cases proving that considerable alterations of the Pons Varolii and Medulla Oblongata may exist without producing paralysis either of sensibility or of voluntary movements.*

We have collected a large number of such cases, mostly relative to the Medulla Oblongata. It is strange that this nervous centre, which is considered so much more important than any other, is, so frequently, and, sometimes, so much altered, without producing either death or even a decided paralysis.†

† The doctrine, now admitted by all Physiologists, as regards the existence, in the Medulla Oblongata, of a small quantity of nervous matter, endowed with the faculty of producing the respiratory movements, is not correct. I have already tried, in the inaugural dissertation of one my pupils, Dr. Benjamin Coste (*Rech. et Observ. sur le role de l'encéphale dans la respiration.* Paris, 1851), to prove that other parts of the nervous centres, besides the Medulla Oblongata, are employed in the production of respiratory movements. I intend to publish, soon, a paper, in which I will

It is strange, also, that slow alterations of the Pons Varolii, which is considered notably less useful, than the preceding organ, produce much more frequently a paralysis, and, this even, when these alterations are much less considerable than alterations of the Medulla Oblongata, which do not produce paralysis.

I need not say that very rapid or sudden alterations, kill more quickly when developed in the Medulla Oblongata than in the Pons Varolii.

I will relate, here, as specimens, some few cases of alteration of both these organs, to show how far such alterations may go, without producing paralysis.

Case I.—A patient had had,—only during the last days of her life,—all the symptoms of a cerebral compression, such as suspension of intelligence, stertorous respiration, sometimes deep groaning, slight spasms and involuntary movements.

There was no alteration of the brain and cerebellum. The volume of the Pons Varolii was almost doubled. It contained an encysted tubercle, with a smooth surface, not adhering to the nervous matter. The size of the cyst was that of a big walnut. A part of the tubercle was dense and lardaceous, the centre was softer.*

Case II.—A man, aged 63, died of acute pneumonia. He had been epileptic for 12 years; each fit began with very violent hiccups, lasting one or two minutes, and accompanied with a sensation of a ball going up from the stomach to the pharynx. All the liquids, that were then given to him, were violently expelled. To this state succeeded a loss of consciousness, which lasted two or three minutes, and then all the accidents passed away. This convulsive and momentary spasm used to come every 15 days, and some physicians had considered it as very different from epilepsy. During the fits the loss of sensibility was complete.

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try to show: 1st. That almost all the parts of the cerebro-spinal axis, have a share in the production of respiratory movements; 2d. That there is no part absolutely necessary for the production of these movements; 3d. That, even in some warm blooded animals, respiratory movements, *i. e.*, inspiration and expiration, may take place after decapitation, which removes the whole Medulla Oblongata, together with the remainder of the encephalon.

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* Traité de la maladie scrofuleuse, par Lepelletier, p. 129.

The brain and the cerebellum were normal; but, in the middle of the substance of the medulla oblongata, two tubercles were found: one as large as a small walnut, and the other the size of a filbert, they were adhering to each other, and each of them was in a very thin cyst. The medullary substance around them was not altered.†

These two cases show that, although there is a diminution in the number of fibres, which establish a communication between the spinal cord and the parts of the encephalon anterior to the medulla oblongata and to the Pons Varolii, sensibility and voluntary movements may continue to exist, at least until a short time before death. We are led by the fact that there is a number of fibres which are then destroyed in a part of their length, to admit one of the two following opinions.

1st. The nerve-fibres of the Pons and the medulla oblongata are not necessary channels of communication, between the spinal cord and the parts of the encephalon anterior to the Pons. I cannot admit such an opinion. It overthrows almost all that is taught by physiology and pathology, as regards the nervous centres.

2d. We can explain the facts by another hypothesis. Suppose that the divided extremities of the destroyed fibres, become connected with either the tails or the envelope of some caudate or bipolar cells, and that by the means of the fibres, originating from these cells, communication may continue, from below upwards and from above downwards. If such a reunion takes place, between the extremities of divided fibres, below and above the seat of the compression or of the alteration, although there is no decided paralysis, there must be much inaccuracy in the action of the will on the muscles, because certain muscles must be put in action at a time where the will does not wish for their contraction. As to sensations, if their intensity is not diminished, there must be a diminution in the faculty of determining from where an impression comes. These two kinds of changes, in voluntary movements and in sensibility, are very frequent in incipient paralysis, whatever may be the seat, in the encephalon, of the alteration which causes it. Very likely these changes are produced by the same anatomical causes, whatever is the seat of the disease.

It may be said that when a tumour presses upon a nervous centre, it may not destroy the fibres and merely separate them one from the other and render them thinner. This certainly ought to take place;

† This case was recorded by Gendrin. See *Traité des Maladies de la Moelle épin.* par Ollivier d'Angers. Vol. II, p. 518.

but, besides this, a partial destruction must also exist, in cases where a tumour becomes extremely large, and particularly if it exists in a relatively narrow place, as is the case, in the vertebral canal, for the medulla oblongata and a part of the Pons Varolii, compared to other parts of the encephalon.

I wish I had room here to publish the very interesting and numerous cases I have collected,* and which show that the Pons Varolii and the Medulla Oblongata may be compressed, and sometimes as much as to be reduced to one half or one third of their size, if not more, although there is hardly any paralysis produced, except during the last days of life. These facts are extremely interesting in a pathological as well as in a physiological point of view.

There is one thing which appears to be well proved by these facts, and especially by those in which a layer only of white matter has been left either in the Medulla Oblongata or in the Pons Varolii, it is that these organs are not the centre, neither an important part of the centre, for volition or for perception of the sensitive impressions; because if they were, these faculties should be lost. We may understand that the power of transmission of nervous actions (either for sensations or for volitions) may be kept in a part where there remains only some fibres, but the higher faculties of Perception and Volition would, certainly, be destroyed, had they their seat in organs so much altered as the Pons and the Medulla Oblongata, sometimes, are.

As these pathological cases might be considered as proving that communications, between the spinal cord and the parts of the encephalon anterior to the Pons, are not necessary for the performance of voluntary movements and for the perception of sensitive impressions, and, as such a conclusion might also be drawn from the result of certain experiments, recently published, I think I ought to examine here these experiments. It is certainly important to make this examination and to show that such a conclusion is not right, because, were it admitted to be correct, all the researches and conclusions exposed in the three first sections of this paper, about the region where the decussation of the

* With the exception of two or three of these cases, I have borrowed them from modern writers. As a guarantee of exactitude of observation, I give here a list of the most known among these writers: Bouillaud, Cruveilhier, Gendrin, J. Cloquet, Duverney, Gama, Esquirol, Stanley, Lieutaud, Lenhossek, Romberg, Abercombie, Velpeau, Guersant, Rilliet, Barthez, Bayle, Hutin, Pariset, Burnet, Ollivier d'Angers, Lebert.

voluntary motor and sensitive nerve-fibres take place in the nervous centres, would be of no usefulness and no meaning at all.

Two talented Physiologists, Drs. Vulpian and Philipeaux,† relate some experiments, in one of which they had divided transversely a lateral half of the medulla oblongata, about one line in front of the nib of the *calamus scriptorius*. The result, as stated by the authors, was : conservation of the voluntary movements and of sensibility in the two sides of the body.

As regards voluntary movements, the experimenters themselves say that the animals could not stand on their feet, but they consider as a voluntary action, a great agitation of the four limbs, consisting in alternative flexions and extensions. I will, first, remark that the experiment could not prove much about voluntary movements, because it had been made on the decussation of the pyramids, leaving there, undivided, a number of voluntary motor fibres belonging to the two sides of the body. We certainly might consider that the agitation was voluntary and produced by the undivided fibres. But we have found that if the lateral hemisection of the medulla oblongata is made a little higher, *i. e.* above the decussation, so that, only the voluntary motor fibres of the opposite side of the body have been divided, there is, also, agitation on both sides, though much more on the side of the section. But this agitation is not a voluntary action ; it is convulsive as I will presently try to show. Before any division of the medulla oblongata, and, even, before having laid it bare, the mere fact of having divided a number of the muscles of the posterior part of the neck, as Longet has found, is followed by a very great agitation in the whole body, at every time the animal attempts to move.

This trouble comes from the fact that the head, being drawn, by the contraction of the anterior muscles of the neck, towards the sternum, the medulla oblongata is drawn upwards and excited, and so are the spinal cord and its nerves. Now, after the section of a lateral half of the Medulla Oblongata, this irritation of all the intra-spinal nervous system continues and only changes somewhat in nature, precisely because the will cannot act, as well as before, to diminish the agitation. This is one cause of agitation, but not the only one ; there is another in the irritation existing in the wound. Besides, the animal, upon which a section of a lateral half of the Medulla Oblongata has been

† Essai sur l'orig. de plus. paires des nerfs craniens, 1853, p. 54.

performed, is attacked with this peculiar and so curious convulsive affection, which manifests itself by a rotatory movement, (see my *Exper. Researches*, p. 18 to 23) and we well know that, in man, this rotation is never voluntary, and that when it takes place in a man who has his consciousness entire, the rotation occurs, although the will tries to prevent it. Therefore the agitation of an animal, upon which a lateral half of the Medulla Oblongata has been cut, cannot be considered as a voluntary action. It may be partly voluntary, in one side of the body, when the section is made above the decussation of the pyramids, but then we have merely a confirmation of the facts and conclusions exposed in the first sections of this paper.

As to sensibility, I need not say much. I had myself published, long ago, that a section of a lateral half of the Medulla Oblongata, above the roots of the pneumo-gastric nerves, does not appear to destroy sensibility, if we decide that this faculty exists, when there are cries and agitation, as have done Drs. Vulpian and Philipeaux. I had gone farther and shown that if, instead of merely cutting a lateral half of the Medulla Oblongata, we divide this organ entirely, where it unites with the Pons Varolii, (as we have already said in a preceding section of this paper) sensibility *appears* to exist everywhere, *i. e.* agitation and cries are observed after every excitation. But, as long as it will not be proved that cries and agitation are not mere reflex actions, we are entitled to consider them as such. Drs. V. and P. say that the animals, upon which they had cut a lateral half of the Medulla Oblongata, appeared to be more sensitive than normally. We agree partly with them: there is, but on one side only of the body, an apparent increase of sensibility, and this side is the side of the operation, whether it is performed upon the Medulla Oblongata, or upon the spinal cord, as we have said in the first section of this paper.

The same results are obtained, as regards sensibility, whether the hemisection is made on the Medulla Oblongata or on the Pons Varolii.

It is known that there is a question, connected with our subject, which has been the cause of great discussion between Flourens, Calmeil, Ollivier d'Angers, Longet and others. This *quæstio vexata* is no more a question, after what we have said. The question was whether the medulla oblongata has a *crossed* action for sensibility and voluntary movement. The solution is, I believe, given in the first section of this paper, and, as to the reasons for which there has been discrepancies of opinions, what we have just said of the experiments of Drs. Vulpian

and Philipeaux, explains how erroneous and contrary deductions may have been obtained.

To conclude on this subject, I will say that the experiments of these two physiologists and the pathological cases, in which, although there was a considerable alteration of the Pons or of the Medulla Oblongata, no decided paralysis existed,—are certainly not able to prove against our admitting that a communication, by voluntary motor and by sensitive fibres, is necessary between the spinal cord and the parts of the encephalon anterior to the Pons. The number of fibres establishing the communication may diminish, without any notable diminution in the intensity of sensations and in the strength of voluntary movements. But, it seems that there ought to be an alteration in the harmony and regularity of the voluntary movements, and in the power of judging from what place comes a sensitive impression.

5. *Cases in which an alteration in the two sides of the Pons Varolii appeared to have produced a paralysis only in one side of the body.*

I know of no case where the Pons, being much and equally altered, on both sides, there has been a mere hemiplegia; but there are some cases, in which an alteration, more considerable on one side than in the other, has produced paralysis in one side only of the body, which side has been that opposed to the most altered in the Pons. Such facts cannot prove more than the facts above related, in which an alteration has existed on both sides of the Pons without producing paralysis.

6. *Cases in which an alteration existing in one side of the Pons Varolii, or in the neighboring parts, appears to have produced paralysis in both sides of the body.*

There are some cases of this kind on record; but they cannot prove much, because an alteration in structure, sufficient to produce paralysis, may extend to a certain distance from the side of the Pons, where is a tumour, to the other side. Now, many alterations may escape the search, made with the naked eye, and, as a tumour is found, observers have thought its existence was sufficient to explain the symptoms observed before death.

I will give here only a short analysis of two cases of this kind, which I find in the remarkable work of my friend, Prof. Lebert, on cancer.*

* Traité pratique des maladies cancéreuses, 1851, p. 806.

Case I.—Headache, blindness, pupils dilated, weakness and rigidity of the limbs, tetaniform contracture of the trunk, convulsions, respiration difficult and noisy, speech embarrassed, death.

Tumour of the anterior part of the left lobe of the cerebellum, which had pushed the Pons Varolii and made, in its anterior part, a cavity, in which it was lodged.

Case II.—Cephalalgia; not long before death, hemiplegia left side; incomplete paralysis of the right side; contracture, frequent shaking; lancinating and great pain in the left limbs; at last, involuntary expulsion of urine and fœcal matters.

A tumour, not larger than a pea, is found in the middle and inferior part of the right side of the Pons.

No physician, acquainted with nervous diseases, will believe that in such a case there was no other alteration, besides the slight displacement or compression of nervous matter, in the immediate neighborhood of so small a tumour. The following fact will show that when an alteration appears to exist only in one side of the encephalon, when there has been paralysis, in the two sides of the body, the microscope is necessary to decide if there is no other alteration more able to give us the reason of the double paralysis.

This fact is so important, that I will give it with all the details, and in the very words of Prof. Hughes Bennett, who has related it. "Another well remarked case was that of a man who entered the Infirmary, under Dr. Paterson, in 1842. All the symptoms of acute softening were present; paralysis of the left side, including rigidity and contraction of the left arm, dullness of intellect, and tonic spasms of the muscles of the mouth and neck. The right side was also affected in a slighter degree. As the case excited considerable interest, great care was taken in examining the brain. When the lateral ventricles were opened, it became a question whether the right corpus striatum was softened. Several persons applied their fingers, and endeavored to ascertain the point. As the manual examination proceeded, the normal consistence of the part diminished, until at length it presented all the appearance of pultaceous softening. In this state it was shown to Dr. Paterson, who naturally enough considered it to be the result of disease. I differed from him in opinion: first, because I had carefully observed the gradual increase of the softening in the manner alluded to; and secondly, because disease of the corpus striatum, in one side

of the brain, could not have explained the well marked symptoms which existed on both sides of the body. When the Pons Varolii was bisected, Dr. Peacock, who conducted the examination, conceived it to be softened; others who examined it, could perceive no difference in the texture; its color and consistence were unchanged. Reasoning from the symptoms, the lesion was very likely to exist. But how, it was argued, could a judgment be formed; we ought to reason from facts, not theories? Here, then, was an evident lesion of the corpus striatum, which explained nothing, and a problematical lesion of the Pons Varolii, which, however, did it exist, would satisfactorily account for the symptoms. In this state of uncertainiy the microscope was sent for, and I demonstrated, and made evident to Drs. Paterson, Peacock, and all the students present, that the corpus striatum contained no granular corpuscles, whilst in the Pons Varolii they were very abundant. I have endeavored to describe what took place on this occasion, from which it must be evident that had not the microscope been appealed to, the right corpus striatum would have been pronounced softened; whilst the real lesion in the Pons Varolii mght have escaped observation. Under such circumstances, this case would have added another to the inexplicable observations with which the records of nervous diseases abound."*

After such a fact, we, certainly, are entitled to conclude, that the cases, in which it is said that an alteration, only in one side of the Pons, has produced paralysis, in both sides of the body, cannot prove anything, because the microscope has not shown that the other side of the Pons was not altered.

7. *Cases in which an alteration, in one side of the Pons Varolii or of the neighboring parts, has produced paralysis, in the same side of the body.*

I regret not to have room enough to treat at length this subject. I will merely give, here, one only the four or five cases that are on record, as far as I know; then I will relate, shortly, the different modes of explanation of these facts, and, at last, I will show that these cases cannot be opposed to what I have said, as regards the place of decussation of motor and sensitive nerve-fibres.

Case.—Hemiplegia of the left side, without loss of sensation in the

* Monthly Jour. of Med. Science. April 1851, p. 365.

arm and leg, but in the left side of the face both sensation and motion were entirely lost. Loss of hearing in the left ear.

A tumour was found in the left side of the Pons Varolii, which compressed the origin of the 5th and 7th nerves against the base of the skull. The tumour was of the size of a walnut, of a firm consistence, and extended into the left crus cerebelli.*

This case is positive, and the co-existence of the paralysis in the face and the body, on the same side, point out, at once, a striking difference between the ordinary cases and this *extraordinary* one. In the ordinary cases, paralysis exists on the side of the alteration in the face, and in the opposite side of the trunk and limbs.

I will remark that in this case there was no paralysis of sensibility in the limbs, and I will add that it has been so, in all the cases, but one, that I know, in which paralysis has existed on the same side as the alteration.

Now, how to explain these facts?

1st. There is an explanation to which we are naturally led: it is, that in the men spoken of in these cases, there was no decussation at all of the sensitive and voluntary motor nerve fibres. This would explain, not only the cases relative to the Pons, but also the cases in which alterations have existed in one side of the cerebrum, of the cerebellum, of the corpora striata, or of the thalami optici, and in which hemiplegia has existed on the same side.

Longet says that, sometimes, he has not found any appearance of decussation of the pyramids, in man, and I have also made the same observation on animals.

2d. It may be that the decussation of voluntary motor and sensitive nerve-fibres, instead of taking place as usually, take place in some men, only in front of the Pons, between the corpora quadrigemina and the crura cerebri.

3d. It may be, as it results from what we have related above, and what has been found by Prof. H. Bennett, that the true alteration which really produces the paralysis, and which can be detected only with the microscope, is in the side of the encephalon, opposite to the side where an alteration is seen, with the naked eye.

Now, I ought to say that, by examining the circumstances of the cases, in which there has been only a paralysis of movement, I am led

* Stanley in Lond. Med. Gazette. Vol. I.

to think that there was no decussation of the pyramids, and that the decussation of the voluntary motor fibres either did not exist at all, in these cases, or existed anteriorly to the Pons. As to the case, in which there was together a paralysis of movement and of sensibility, I think it may be explained by what results from Prof. Bennett's researches.

Both the theories actually proposed about the place where exists the decussation of the voluntary motor and of the sensitive nerve-fibres, one which is the theory of Longet, Foville, Valentin and others, and the second which is mine,—are, as much, in apparent opposition with these cases, (of paralysis in the same side where is the alteration) one than the other.

If it were true, as admitted by Foville, Valentin, Longet, etc., that the decussation of the voluntary motor and sensitive nerve-fibres, takes place partly in the Pons and partly behind and before this organ, a considerable alteration of its sides, producing paralysis, should produce it, in both sides of the body; so that, if paralysis exists, only in one side, be it the side of the alteration or the other, the fact, in both cases, appears as much in opposition with the theory.

But I ought to say that if, as it seems to be there are, positively, cases of paralysis, in one side of the body, with considerable alteration of the same side of the Pons and no alteration whatever in the opposite side of the encephalon, these cases can prove nothing, but that there was no decussation of voluntary motor or sensitive nerve-fibres, existing behind or in the part altered, in the subjects of these cases. Therefore, no conclusion can be drawn against any of the theories, the object of which is to show where the decussation takes place, when it exists.

8. *Cases and experiments which appear to prove that there are, in various parts of the encephalon and in the spinal cord, motor nerve-fibres, which are not voluntary motor.*

As to the spinal cord, the existence, in it, of fibres, which are motor but not voluntary motor, has already been pointed out by some physiologists, and I have, already, spoken of this fact, in another section of this paper. As it is not necessary for my object here to demonstrate the fact, I will merely state that, I believe, it is certainly true that there are such fibres. As to the encephalon, not only the doctrine of the existence of such fibres is a new one, but the proofs themselves,

upon which it is grounded, are mostly new or presented, here, in a new light.

For a long while, since Hippocrates, it has been admitted that in the wounds of the brain, the convulsions are always in the injured side, while paralysis is in the opposite side. Haller, though inclined to admit this doctrine, had remained in doubt about it.

Flourens* thought he had decided the question, and he gave the following conclusions :

1st. The cerebral lobes and the cerebellum never give convulsions.

2d. The quadrigeminal tubercles give convulsions in the opposite side.

3d. The Medulla Oblongata and spinal cord give convulsions in the injured side.

Flourens has been led to erroneous conclusions, partly, because he has not taken notice of what has been observed in man, partly, because he has particularly experimented on birds.

Burdach, according to J. Mueller,† has given the following statistics : out of 268 cases of alteration on one side of the encephalon, there has been 10 cases of paralysis in both sides of the body and 258 of hemiplegia, of which 15 in the side of the alteration. Convulsions took place in 25 cases, in the side of the alteration, and in only 3 cases in the opposite side.

From many facts recorded by Andral, Rochoux, Rostan, Abercrombie, Bright, Bouillaud, Lallemand and Romberg, it results that convulsions in the side of the alteration appear to be less frequent than convulsions in the opposite side. So that the results arrived at by Burdach, are opposed to the results of these more recent writers. But whatever may be about this, it is sufficient, for my object now, that it is certain that convulsions may take place, either in the side where is the alteration or injury of the encephalon, or in the opposite side.

Now, there are other and very curious facts which also prove that contractions of certain muscles of the body may take place either on the side of the body corresponding to the injured side of the encephalon, or on the opposite side, exactly as in the cases of ordinary convulsions, as I have just said. And, in fact, this might have been foreseen had it been known, that a convulsive state of certain muscles is the cause of the phenomena of which I will now say a few words.

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* Loco cit., p. 120.

† Manuel de Physiol. Ed. Littré, 1851, Vol. I, p. 783.

If a puncture or rather a slight section is made, on mammals, in different parts of the encephalon, we see quite different effects, according to the part which has been injured. The animal *turns* round or *rolls* over itself, (see my paper on *turning* and *rolling* in *Exp. researches*, p. 18) and this, as I will prove, elsewhere, in consequence mostly of local convulsions. Turning or rolling take place after an injury of the right side, sometimes on the right, sometimes on the left side of the body, as it will be seen in the following list, in which, to avoid repetitions, we will say merely left or right, according to the side of turning or rolling of the man or animal. The sections of the parts of the encephalon, I will suppose to have all been made, on the right side of the encephalon.

1st. Anterior part of the thalamus opticus, according to Schiff. Right.

2d. Posterior part of the thalamus opticus, also according to Schiff. Left.

3d. Some parts of the crus cerebri, near the thalamus opticus, according to my experiments.—Left.

4th. The hind parts of the crus cerebri.—Right.

5th. The testes and nates, according to Flourens.—Right.

6th. Anterior and superior part of the Pons Varolii, according to my experiments.—Left.

7th. Anterior part of the processus cerebelli ad pontem.—Left.

8th. Posterior part of the processus cerebelli ad pontem, according to Magendie.—Right.

9th. Place of insertion of the auditive nerve or this nerve itself, according to my experiments.—Right.

10th. Place of insertion of the facial nerve or the mere drawing out of this nerve, according to myself and my friend Dr. Martin-Magron. Right.

11th. The medulla oblongata, according to my experiments.—Right or left, according to the place and to the extent of the injury.

12th. The spinal cord near the medulla oblongata, according to my experiments.—Left.

In almost all these experiments, turning or rolling exist on the side where there are convulsions in certain muscles. The spasms exist in all cases, in the muscles of the neck, frequently in those of the trunk and sometimes in those of the limbs. The convulsions of the muscles of the neck and trunk are sufficient, without the assistance of the limbs

to produce rolling, as I have frequently seen, after the amputation of the four limbs.

It seems quite certain from these two series of facts (*i. e.* pure convulsions in one side of the body and the local spasms coexisting with turning and rolling), that convulsions may be produced in muscles of one side of the body, by the same alteration which produces paralysis in the opposite side. From this it results that there are two different sets of motor nerve-fibres, which appear to originate from the same place, in many parts of the encephalon, one set being composed of voluntary motor fibres, which then become paralysed, and the other set composed of motor fibres, which are not voluntary motor, because, were they so, their alteration should produce paralysis and we should, therefore, have, then, a paralysis in both sides of the body, although the alteration should exist only in one side of the brain. But what are these fibres, as they cannot be voluntary motor?* They, certainly are able to produce muscular contractions, and, therefore, we are entitled to call them motor, but we do not know whether the irritation acts directly upon them, or if it is by a secondary, that is a reflex action that they become excited. It may be, and this seems very probable, that they are merely reflecto-motor.

But, whatever is the truth about their nature, they are motor and they exist in great number, in all the isthmus of the encephalon, and particularly in the Medulla Oblongata. We can by this fact understand why there are so many motor fibres belonging to the anterior columns of the spinal cord which are not voluntary motor.

9. *Anatomico-pathological dissections which appear to prove that there are nerve-fibres, coming from the spinal cord, which decussate in parts above the Medulla Oblongata.*

The interesting facts, discovered by Dr. Ludwig Türck of Vienna†,

* It is well known that no movement is produced in the limbs and trunk, when an excitation of any kind is brought upon the cerebral lobes or even the corpora striata. As the voluntary motor fibres extend in the encephalon, if not into the cerebral lobes, at least, into the corpora striata, it is certain that there, these fibres cannot be irritated by our means of excitation. It may be that these fibres remain so, *unirritable* all along, from these parts of the encephalon to their termination in muscles, and that the nerve-fibres which produce contractions when we irritate them, either in the encephalon, in the spinal cord or in the nerves, belong only to the other class of motor nerves, the existence of which I am now trying to establish.

† See Braithwaite's Restrospect, Amer. Edit. Part 27, p. 344.

showing that, when there is an alteration of a part of the encephalon, the nerve-fibres which go from that part into and along the spinal cord, become very much altered in their structure; these facts have proved that there is a decussation, for these fibres, in the Pons Varolii or in parts before it.* I do not think necessary to discuss at length the value of these researches, in relation to the subject of this paper. I believe it is sufficient to say that, as long as there are other fibres, besides the sensitive and the voluntary motor, which originate from the different parts of the encephalon, nothing in the curious facts, described by Dr. Türck, may prove against our admitting, that the decussation of the sensitive and voluntary motor fibres, takes place where I have tried to show that it does.

This means of study of Dr. Türck will have the greatest value when (and only then) it will be combined with all the other means that science already possesses, to determine what are the kinds of nerve-fibres existing in the nervous centres, and what is their respective course.

GENERAL CONCLUSIONS.

In this paper we have tried to show:

1st. What is the place of the decussation of the sensitive and voluntary motor nerve-fibres, in the cerebro-spinal axis. The conclusions, on this subject, have already been exposed at the end of the third section.

2d. That, as regards the objections which might be made to my conclusions, relative to the place of decussation of the sensitive and voluntary motor nerve-fibres, I have tried to prove successively:

a. That reflex movements alone, and not sensations and volitions, exist in monsters, deprived of a great part of their cerebro-spinal axis.

* Dr. Türck is disposed to consider the alteration produced, in these cases, in nerve-tubes, as a result of absence of action. I believe this view is partly right, but there is another cause of alteration, which is,—according to what I have tried to show in a paper presented, more than a year ago, to the *Société de Biologie*,—that the nerve-tubes are endowed with capillarity, and that, liquids in which are placed a divided end of them, are absorbed, and conveyed very far in their canal, and, there, altering their contents. The spinal cord may become secondarily affected, in that way, by diseases of the different viscera, and this may prove to be a good means of finding the course of the roots of the nerves in the spinal cord. I had already made some researches about the disposition of these roots, a year ago, and I intend to resume them as soon as possible.

b. That, when the spinal cord, the Medulla Oblongata or the Pons Varolii, are altered, even considerably, sensibility and volition may continue to exist, because there are, still, communications by nerve-fibres through the altered part, between the nerves of the trunk and limbs, and the parts of the encephalon, in front of the Pons.

c. That if the reasons, given by many physiologists, to prove that the Pons Varolii is the seat of the centre for volition, and for perception of sensitive impressions, were true, we should have to admit that the Medulla Oblongata is the centre (or, at least, a part of the centre,) for these faculties, because the same reasons appear to prove so, for this organ, as for the Pons.

d. That very likely these faculties have not their centre, (at least, their principal centre) in the Pons Varolii, and, still less, in the Medulla Oblongata.

e. That it seems that, in some men, the sensitive and voluntary motor nerve-fibres do not decussate at their usual place, and that, in consequence, some rare cases may exist in which an alteration in one side of the Pons Varolii or of the Medulla Oblongata, will produce paralysis of the same side of the body.

f. That there appears to be, in many places of the encephalon, nerve-fibres, which are not voluntary motor, and which, nevertheless, go to muscles, either on the same side of the body as the side of the encephalon, from which they originate, or on the opposite side, and that these muscular nerve-fibres are able to produce convulsions, when they are irritated by an injury or an alteration in the encephalon, so that convulsions may take place either on the paralysed side or in the other.

g. The results of the researches of Dr. Ludwig Türck cannot, in the actual state of science, prove against or in favor of any doctrine relative to the place of decussation of sensitive and voluntary motor nerve-fibres.